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NOVEMBER, 1919

Vol. X, No. 8

ORIGIN OF NEW PRUNE VARIETY

POLYEMBRYONY AND SEX

CEMETERIES SHOULD PRESERVE LIFE RECORDS

DEMOCRACY AND HEREDITY

FIRST BAD HABIT—SUCKING ITS THUMB

FAMILY HISTORY OF COLOR BLINDNESS

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NOTE

Instead of raising the price of membership, and in order to meet the requirements of the War Industries Board, and also because of the drafting of the whole Editorial Staff the July, August and September numbers have been dropped out for 1918 and 1919. Vol. IX contains only 8 instead of 12 numbers, as will also Vol. X.

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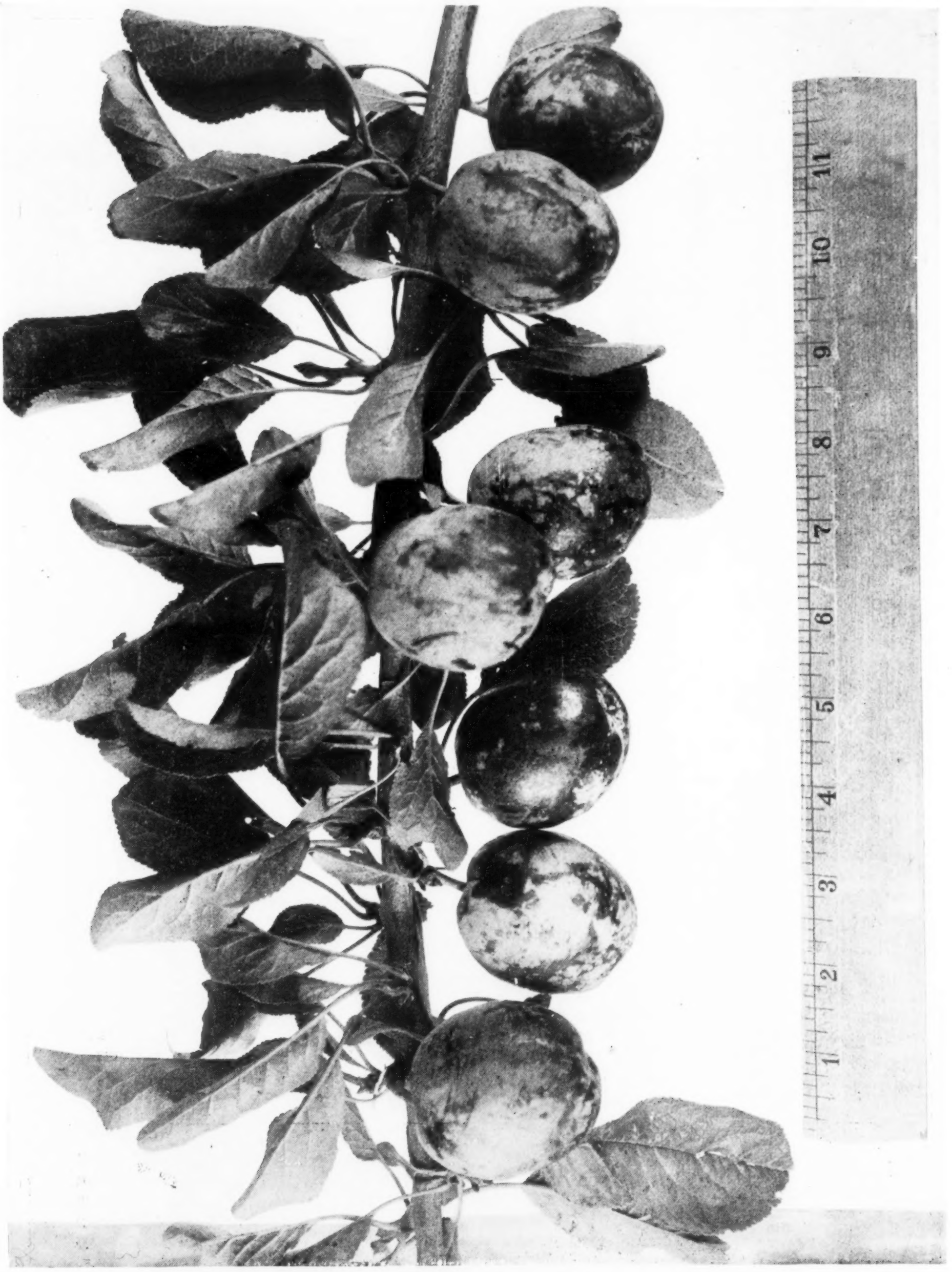
Date of issue of this number, November 24, 1919.



A BRANCH OF THE NEW FRENCH PRUNE TREE

Some typical fruits of the new variety of French prunes are here shown. These fruits (reproduced above to approximately one-half natural size) are about twice the size and weight of the ordinary variety, and are more uniform in all of their characteristics. The trees seem to have a more vigorous growth and develop larger leaves than the parent variety. (Frontispiece.)

ORIGIN OF A NEW AND IMPROVED FRENCH PRUNE VARIETY

A. D. SHAMEL

THE French prune, *Prunus domestica,* commonly called the *petite prune d'Agen* or little French prune, was introduced into California in 1856 by Louis Pellier, a nurseryman of San Jose, who brought the scions from Ville Neuve d'Agen, France, from which place the variety takes its name. The commercial development of this prune in California started about 1880. In 1918, according to the California State Commission of Horticulture, there were 100,721 acres of prune trees in bearing and 34,690 acres of young trees not in bearing. The production amounted to 39,127 tons with an approximate valuation of $5,500,000. The Santa Clara valley is the center of greatest production, having about 60,500 acres of bearing prune trees. In 1919 the very heavy crops, together with the high prices being paid for prunes, seem likely to establish a high record both for the amount and value of the crop.

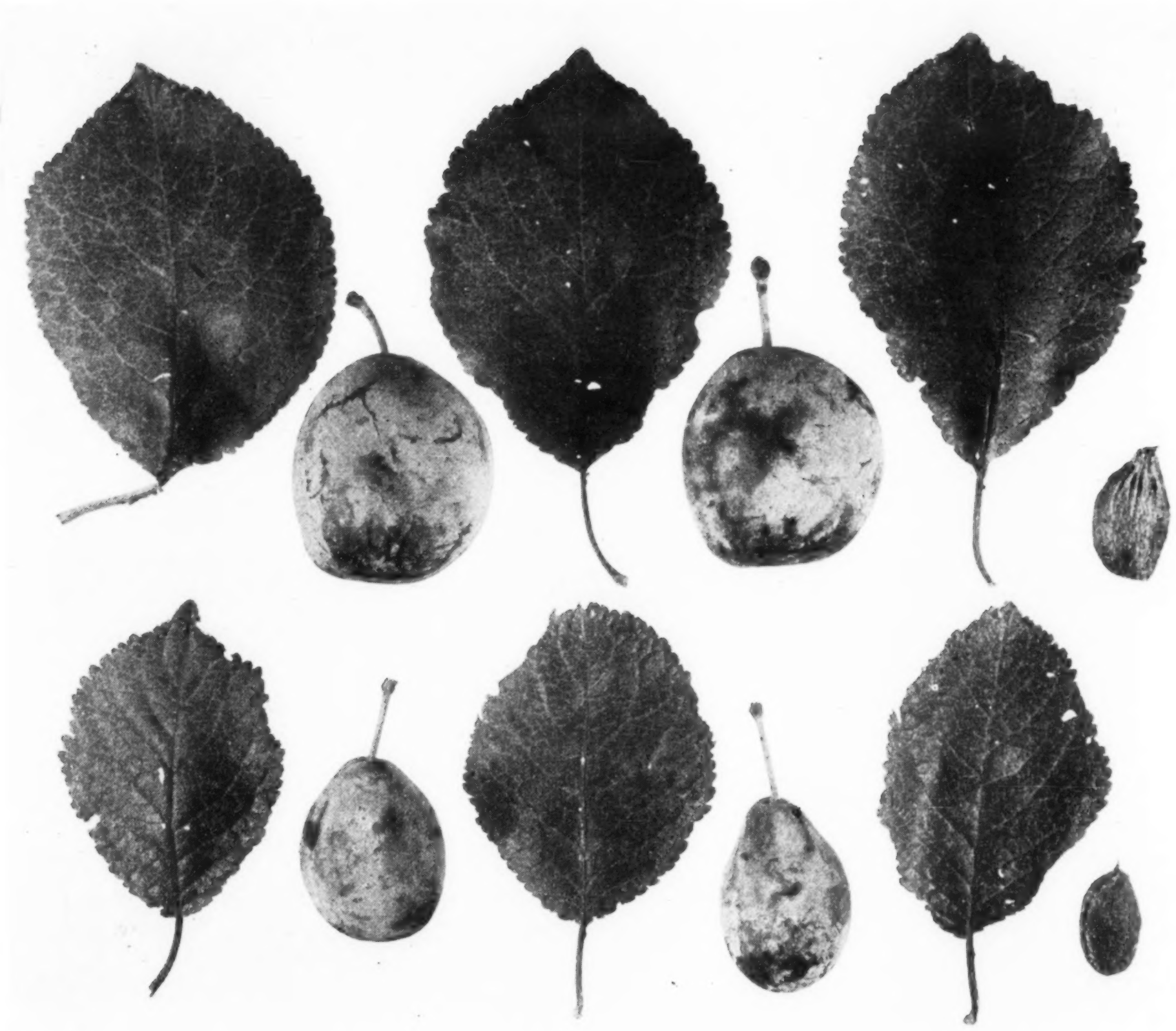
During recent years the prune industry has developed extensively in the Pacific northwest, particularly in the states of Oregon, Washington and Idaho. Formerly, the growth of the prune industry was slow in these districts on account of difficulties in drying the fruits under natural climatic conditions. Recently, the drying of the fruits has been done successfully under controlled conditions with hot, dry air as the source of heat in the evaporators. This development seems likely to lead to an extensive culture of the prune in the northwest, where drying under natural conditions is frequently interfered with by fog and rain.

The Prune d'Agen variety, most generally grown in California, is particularly well adapted for prune production on account of the rich, aromatic flavor of its fruits, the dense, fine texture of the flesh, which gives the cured fruits tenderness both when used as a confection and when slightly cooked, and the smallness of the pits, together with their thinness and smoothness. The dried fruits frequently contain more than 50% of fruit sugars. Correlated with this sugar content is a high degree of spicy prune flavor. The color of the fruits of this variety is a royal purple, which is an important factor in the successful marketing of the dried prunes. The delicious, melting flesh separates easily from the small flat pit.

The trees of this variety are heavy bearers under normal conditions. Some fruit is usually borne the third year after planting. From the fifth year on, the trees may be expected to produce commercial crops. From 200 to 300 pounds are considered to be a satisfactory yield for a full-bearing French prune tree. However, 600 and even 800 pounds have been produced, and a six-year-old tree at Visalia, Cal., is said to have borne 1,102 pounds of fruit in one season.

The tendency to heavy bearing is frequently correlated with small size of fruits, particularly when the crops are not thinned at the proper time. If too much bearing wood is grown, the fruits are likely to be too small for satisfactory commercial purposes. For this reason there has been an active demand among prune growers for a larger French prune having the valuable characteristics of the established variety. Until recently all efforts to achieve this result seem to have been failures. The trouble with the larger fruited varieties, as a rule, is that the fruits are likely to be of poor quality, coarse, and stringy in flesh, or have large, undesirable pits.

Leonard Coates, nurseryman and

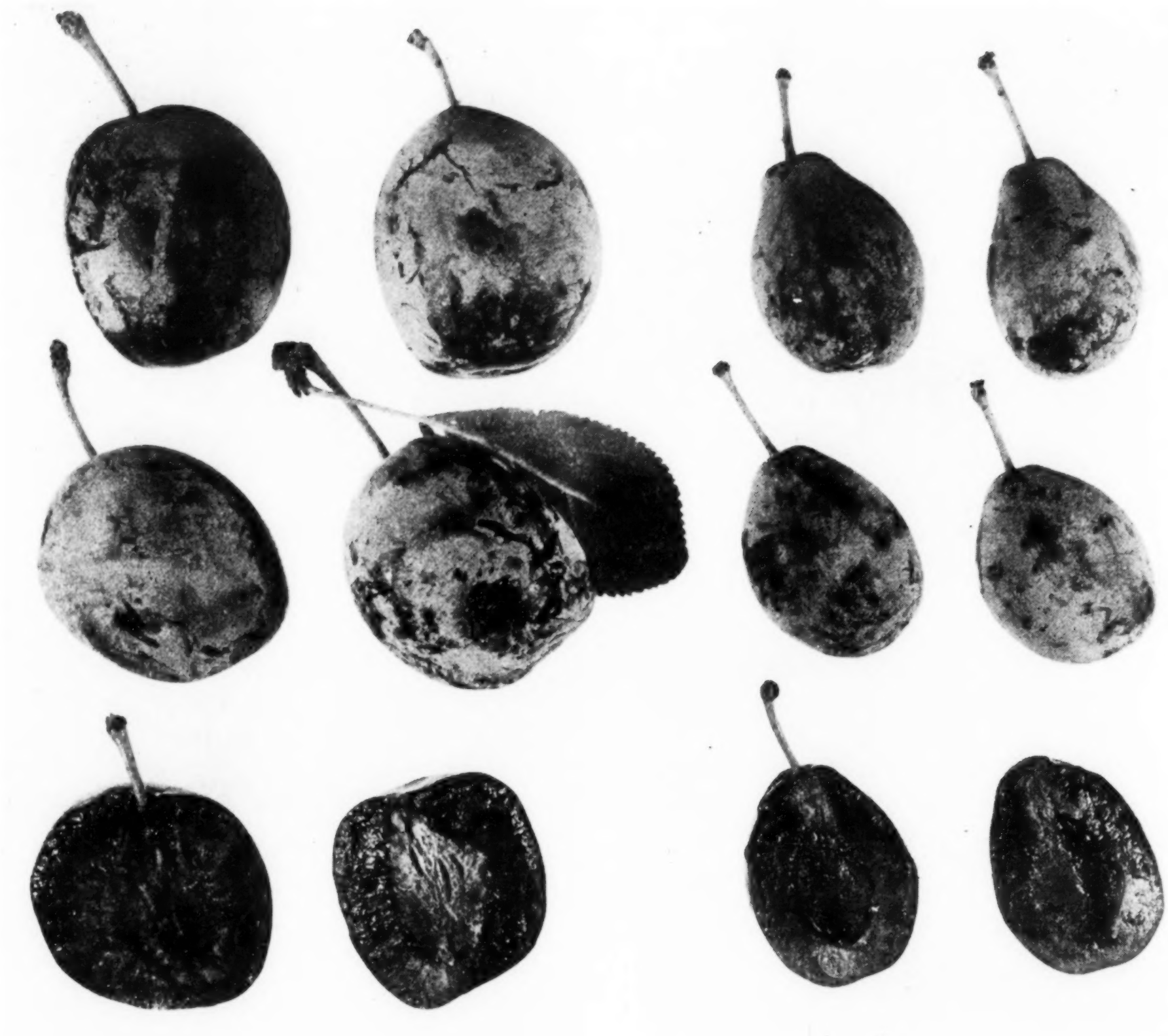


COMPARISON OF NEW AND ORDINARY VARIETIES

Typical leaves, fruits and pits of the two varieties are shown above, those of the new variety at the top and the ordinary variety at the bottom. The former are larger and more uniform than the latter. (Fig. 1.)

fruit grower of Morganhill, Cal., has been studying the problem of securing an improved variety of the French prune for many years. In the course of his experiments he has isolated several strains which differ from each other in one or more clearly recognizable characteristics. No attempt will be made at this time to discuss these various types and their relation to the original variety. This account will be confined to a description of the isolation, propagation and testing in an experimental orchard, of a strain which originated from a bud variation and which promises to be the larger fruited variety so long sought for by prune producers in California.

In 1904, in a French prune tree growing in an orchard near Saratoga, Cal., one branch high up in the tree was found bearing very large fruits. There is no question as to its being a true bud variation. Several grafts were secured from this branch and placed in bearing peach trees in order to secure early evidence as to whether this variation, or bud sport, could be propagated. The fruits produced by these grafts were found to be identical to those borne by the original branch. The large fruits possessed all of the desirable characteristics of the smaller fruits of the ordinary French prune and, in addition, possessed the desired improvement in size.



TYPICAL FRUITS OF THE TWO VARIETIES

The fruits shown in the left half of the picture are of the new French variety, and those on the right are of the ordinary variety. These samples were grown on neighboring trees in the same orchard. Note the cross-sections at the bottom. (Fig. 2.)

In order to give this strain a commercial test Mr. Coates bought 10 acres containing about 1000 peach trees for experimental trials of the large prune variety. These trees were five years old in 1914 at the time of their purchase. The large-fruited French prune variety was budded into every other row of the peach trees with the usual method practiced in top-working citrus and other fruit trees. Mr. Coates believes that budding is preferable to grafting for top-working peach or other stone fruits.

The top-worked trees with the improved French prune strain, called No. 1418 for convenience during the experimental stages, are in alternate rows with the ordinary or other selected strains of the parent variety. In other words, in the 10-acre experimental orchard there is one row of No. 1418 followed by a row of the parent variety, and so on throughout the entire orchard. The conditions are comparative and furnish the basis for a fair comparison of the No. 1418 strain trees with those of the parent variety.

The yield of the No. 1418 trees in the experimental planting has been more than double that of the comparative trees. The No. 1418 fruits are about twice the size and weight of the comparative fruits. They are more uniformly distributed throughout the tree than is the case with the fruits borne by the

comparative trees. Furthermore, the fruits are more uniform in size, shape and other characteristics than are the fruits of the ordinary variety. So uniform are the No. 1418 fruits that they appear to have been graded mechanically as to size as they lie on the ground after falling.

The No. 1418 trees appear to be more vigorous growing and develop larger leaves than do the comparative trees. The leaves of the trees of this strain appear to be thicker and have a tougher feel than do the leaves of the trees of the parent variety. In looking down the rows one notices that the larger trees of the No. 1418 strain, with their more luxurious and abundant foliage, stand out markedly as compared with the trees and leaves of the parent variety.

The fruits of the No. 1418 strain average about 25-30 to the pound as compared with an average of from about 50-60 to the pound as is the case of the fruits of the parent variety. The fruits of the No. 1418 strain drop clearly from the trees and are so firm that in a recent field examination no damage could be observed as a result of their fall. There was little or no bruising or other injuries observed in the fruits on the ground.

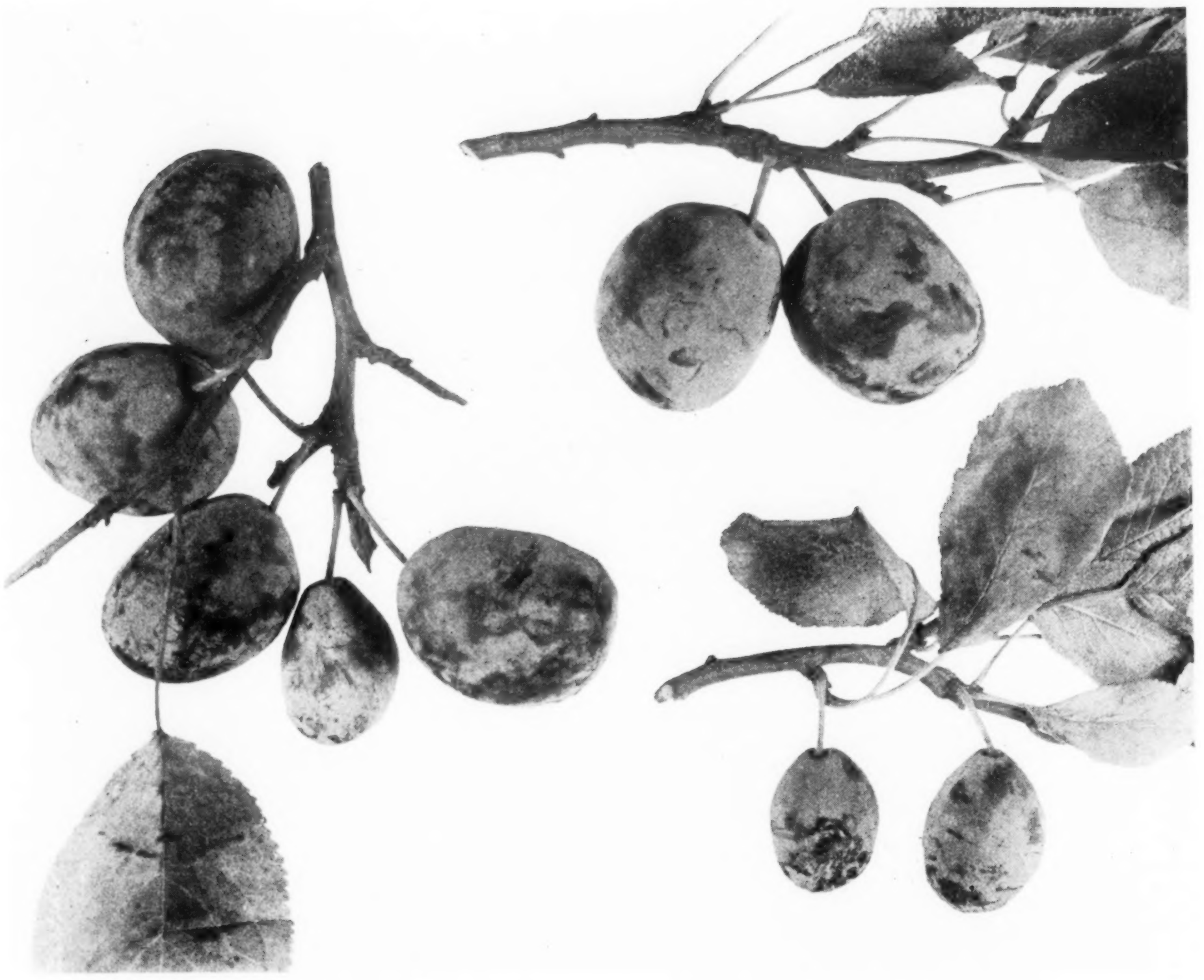
The fruits of the No. 1418 strain ripen at the same time as the fruits of the parent variety. They have the desired rich purple color, and on the drying trays this color shows as a very deep or dark purple. The flavor and other qualities of the No. 1418 fruits are said to be better than those of the parent variety by the manager of the principal drying organization in the district where the experimental orchard is located.

After the prunes are picked up from the ground they are dipped in a boiling lye solution with the aid of a large wire basket. After dipping in the lye solution the fruits are run over a sizing machine which assorts them into three grades according to size. Then the fruits are spread evenly in large shallow wooden trays and allowed to dry in the sun for from eight to ten days. Later, the trays are piled in stacks, where the drying and curing processes continue until finished. The handling of the prunes during the drying period requires care and experience. While kiln-drying is said to be successfully used in some districts, the manager of the plant where the prunes of the No. 1418 strain are dried does not believe it to be a commercial success under his conditions. The curing processes must be so managed as to preserve the rich, glossy color, meaty condition and high flavor essential to good prunes.

As a result of the favorable opinions expressed by propagators, growers, and driers as to the commercial value of the No. 1418 strain, Mr. Coates has decided to introduce it into as many prune-growing districts as possible in order to secure final information as to the range of its adaptability and value for commercial prune production. In this decision he is certainly justified by the experimental evidence in his test orchard thus far. If the improved variety proves to be equally valuable in other districts, it will undoubtedly be the most valuable addition to the commercial prune varieties ever introduced in America.

In carefully studying the individual trees of the new variety Mr. Coates has occasionally observed small branches, and in some cases individual fruit spurs, bearing the ordinary French prunes. The pyriform shape and other characteristics of the French prune, as contrasted with the more round or oval shape of the Coates variety serve to clearly distinguish the two types. While going over some of these trees recently with Mr. Coates, the writer found several similar branches, usually small ones, bearing several fruits which were unmistakably of the parent variety or ordinary French type, while the remainder on the trees were of the large Coates variety. This condition is additional evidence as to the origin of the variety as a bud variation from the ordinary French prune.

It should be noted in this connection that the experimental orchard has not been irrigated thus far. The conditions



THREE BRANCHES FROM THE SAME TREE

The above photograph shows some typical cases of bud variations. All of these branches were cut from the same tree of the new French variety. On the left is a branch bearing four new French prunes and one of the ordinary variety. On the right, the branch at the top has two typical new French prunes, and the one at the bottom, two typical ordinary French prunes. (Fig. 3.)

therefore are more natural than where irrigation is practiced. Furthermore, the fruits of both the Coates and the ordinary French trees were not thinned. While the crops on the trees of the new variety were much greater than those on the trees of the ordinary variety, the new fruits were about twice the size and weight of the ordinary ones.

Twenty years ago the prune was often the subject of many alleged jokes on the part of our breakfast humorists. As a result many of us in college boarding houses and elsewhere looked with disdain upon this healthful and delicious fruit. Those days are gone, never to return. The writer recently paid forty cents for a dish of prunes served on a dining car in California. The prune is no longer a joke. As the price has gone up we have learned to prize it more highly. Prunes are now shipped from California all over the world. They are even exported to that district of France from which the prune d'Agen variety came to us. It is the superior dried fruit, and one which is not only good to eat but has very great therapeutic value as well.

Until recently the writer had never tasted a fresh prune. When fully ripe, as they are picked up for drying the fresh prune is luscious. No other word describes it. To those who have never tasted the fresh prune there is a new delight in store. For those who cannot have this opportunity the stewed dried prune or the delicious prune pie is the next best experience.

POLYEMBRYONY AND SEX[1]

A Study of the Origin and Development of Mixed Broods in Polyembryonic Hymenoptera, and the Ratio in Production of Males to Females.

J. T. PATTERSON
Professor of Zoology, University of Texas

INTRODUCTION

IN ANIMALS the term polyembryony has come to include all cases in which two or more individuals arise from a single egg during the course of its early development. There are two types of polyembryony: (1) Specific polyembryony or the habitual production of multiple embryos in a given species; (2) sporadic polyembryony, or the occasional production of multiple embryos in a species in which the egg typically differentiates into a single individual.

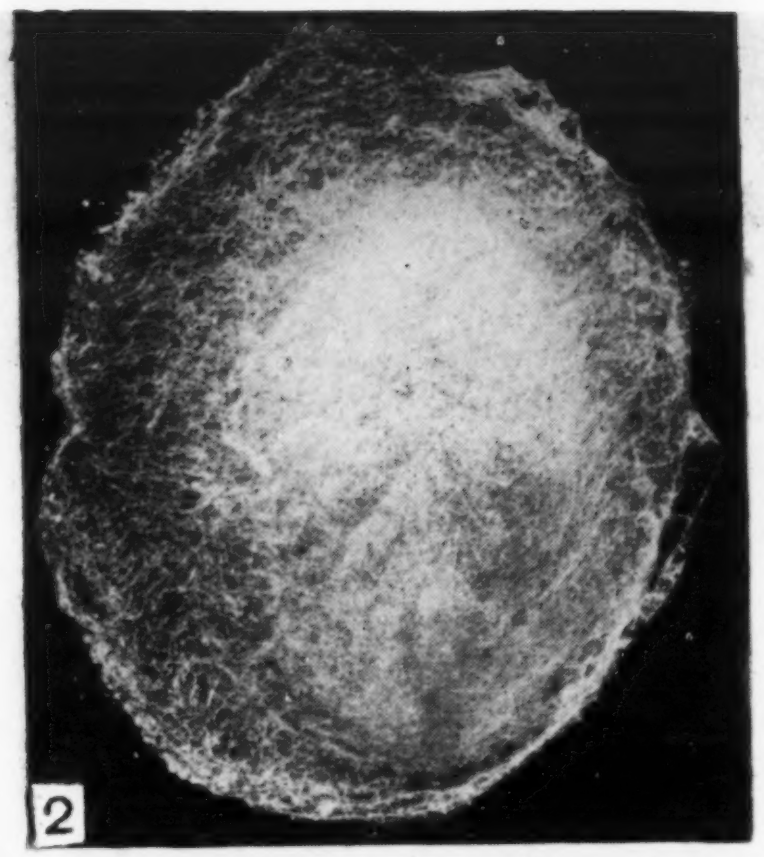
Sporadic polyembryony occurs in many different species among the higher forms. In fact, cases have been recorded for every class of vertebrates. To this type belongs the well-known uniovular, or identical twins of the human species. Such twins are enclosed in a single chorion and are therefore often referred to as monochorial or monozygotic. They are invariably homosexual. In contrast with these are biovular or dizygotic twins, which may be heterosexual. In such cases the two individuals are no more similar than brothers or sisters in a family born at different times, or than the individuals in a litter of a multiparous species. They are often called fraternal twins.

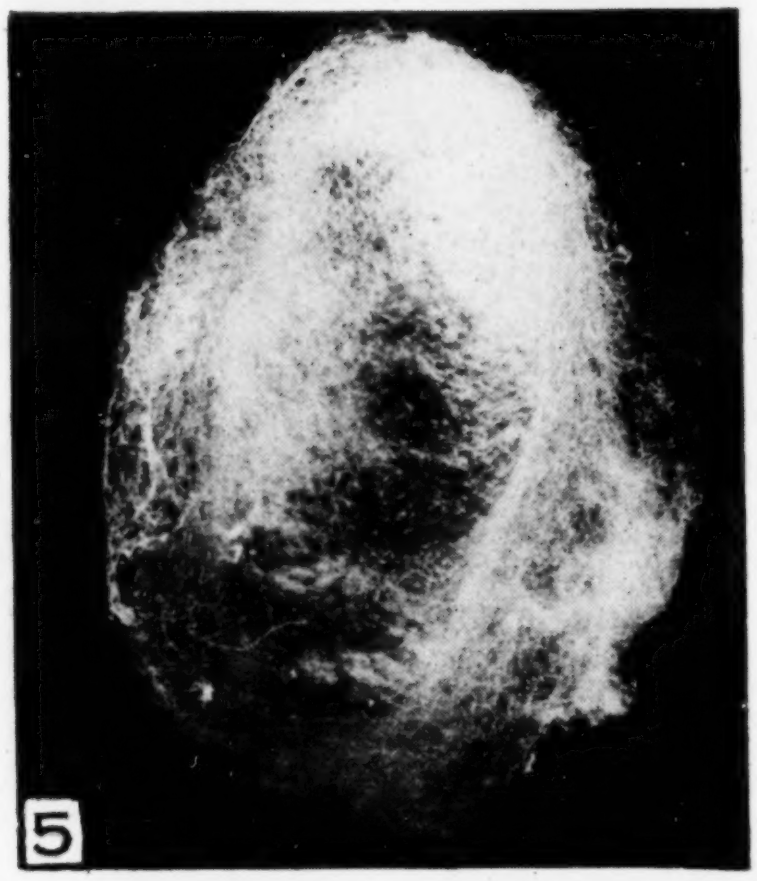
The interesting fact concerning monozygotic twins is their homosexual condition which is explained on the basis of the chromosome hypothesis of sex-determination. According to this hypothesis, if the egg is fertilized by a male-determining sperm the twins will both be male, but if fertilized by a female-determining sperm they will both be female. It is supposed that, after the egg has once been fertilized, sexuality is irrevocably fixed, and, no matter how many individuals may later develop from it, such individuals will be homosexual.

The early data collected on sexes among species exhibiting specific polyembryony also support the chromosome hypothesis. Thus the four embryos which develop from a single egg of the Texas armadillo (*Tatusia novemcincta*) are invariably of the same sex. The same is true for the larger polyembryonic litters of one of the South American armadillos (*T. hybrida*).

The other groups of dioecious species which show specific polyembryony belong to the parasitic hymenoptera. Because of the large number of individuals which develop from a single egg in certain of these insects, data on their sex ratios are of especial interest. Unfortunately, we have but very few exact data on the sex ratios of these insects. Bugnion ('91) was the first investigator to record such data. He reared and studied twenty-one polyembryonic broods of *Ageniaspis* (*Encyrtus*) *fuscicollis*, and found nine female broods and five male broods, and seven broods that contained both males and females. Of the seven mixed broods, three had males and females in about equal numbers, three had a large majority of males, and one had a large majority of females. Marchal ('04) studied sixteen broods of *Polygnotus minutus*. He found eight female broods, six male, and two mixed, one with three females and three males, and the other with three females and one male. Silvestri ('06) found, in one hundred and sixty broods of *Litomastix*, sixty-three female, ninety-two male, and five mixed

[1]Contributions from the Zoological Laboratories, University of Texas, No. 142.





POLYEMBRYONY IN THE CASE OF PARASITIC HYMENOPTERA

1. Newly formed carcass of Autographra, containing parasitic pupae. 2. Newly formed cocoon, containing carcass. 3. Older carcass. 4. Cocoon with normal pupa of Autographra. 5. Cocoon containing pupae which are turning black. 6. Empty carcass showing holes through which parasites have escaped. 7. Gall of Rhopalomyia. 8. Silhouette of female Paracopidosomopsis ovipositing in egg of Autographra. 9. Gall of Walshomyia. (Fig. 4.)

broods. He does not give the numbers of individuals for any of these broods.

In commenting upon the data recorded by Bugnion for *Ageniaspis,* Marchal points out that it is difficult to account for the four broods in which a large majority of individuals belong to the one or the other sex. He believes that such broods must arise from two (or more) eggs. He further believes that should two eggs, one fertilized and the other not, be laid in the egg of the host at slightly different times, the one first deposited might gain the upper hand in the matter of food, and thus prevent all but a few of the individuals arising from the second parasitic egg from reaching maturity.

During the past few years I have been able to study in detail the polyembryonic broods of three distinct species of parasitic hymenoptera. The first species studied was *Copidosoma gelechiae,* which is found in the larvae of the Solidago gall moth, *Gnorimoschema salinaris.* This moth makes the ellipsoidal galls on the stem of the marsh goldenrod, *Solidago sempervirens.* The second species investigated was *Paracopidosomopsis floridanus,* which lays its egg in the egg of the common cabbage looper, *Autographa brassicae.* Recently I have studied the broods of *Platygaster sp.*,[2] which is found in the larvae of two dipterous gall-makers of the mountain cedar (*Sabina sabinoides*). The two hosts are *Walshomyia texana* and *Rhopalomyia sabinae.* We shall present the data on the sexes of these three species in the order named.

SEX RATIOS IN COPIDOSOMA

A total of one hundred and sixty-two broods of this species have been reared and the sexes noted. Ninety of these were female broods, sixty-two male, and ten mixed. I shall here give only the summary of these data, since the details have been published elsewhere (Patterson, '15). On the per cent basis these data show that 55.56% of all broods are female, 38.27% male, and 6.17% mixed. The total number of individuals is 31,001, of which 63.41% are females and 36.59% males. The average number of individuals in female broods (198) is higher than in male broods (175). However, the range in the number of individuals in the several broods (from 25 to 395 in the female, and from 41 to 345 in the male) is such as to indicate that the average per brood is of little significance in determining the true sex ratio for the species. Obviously this must be based on the number of male and female polyembryonic broods. It would be easy to determine this ratio were it not for the uncertainty of the origin of some broods. If it be assumed, as all previous investigators have done, that each mixed brood is at least dizygotic, then, in accordance with the law of probability, some unmixed broods must also be of dual origin. Worked out on this basis, it is found that the ratio of females to males is 106 to 76, or a sex ratio of approximately 3:2.

The interesting point brought out in the study of these insects is the ratio of males to females in the ten mixed broods. These are listed in the following table:

TABLE I.—*Mixed Broods of Copidosoma.*

Brood	No. of individuals	Females	Males
1*	89	20	69
2	162	153	9
3	172	92	80
4	207	126	81
5	216	176	40
6	235	223	12
7	241	161	80
8	300	235	65
9	304	292	12
10	337	316	21

* An incomplete brood, owing to the fact that some of the larvae and pupae had been destroyed by a dipterous larva.

In each of the nine complete lots listed in the table the number of females is greater than the number of

[2] I am indebted to Dr. C. T. Brues, who kindly informs me that this species belongs to the genus Platygaster. I am also indebted to Dr. E. P. Felt for naming and describing the two hosts here referred to.

males, but in five of the cases (Broods 3, 4, 5, 7, 8) this difference is not so great but that the origin of each lot can be explained on the assumption that two parasitic eggs have been deposited in the egg of the host. In each of the four remaining broods (2, 6, 9, 10) females are greatly in excess of males, so much so that it is difficult to explain the origin of such broods on the basis of two eggs. It was this evidence, although meager in amount, that suggested the possibility that such mixed broods might be monozygotic, that is, the origin of males and females from a single fertilized egg.

SEX RATIOS IN PARACOPIDOSOMOPSIS

Three hundred and sixty-four parasitized carcasses of the *Autographa* moth have been collected from the field. Many broods of parasites have been reared from these in the laboratory. The sexes of the individuals in 177 cases have been studied. The results of these studies show that only 3 broods, or 1.7%, are pure female broods; 20, or 11.3%, are pure male broods; and 154, or 87%, are mixed.

The three female broods contained 1,089, 1,306 and 1,859 individuals, respectively. The male broods vary from 385 to 1,588 individuals, with an average of about 1,000. The mixed broods vary from 545 to 2,028 individuals, with an average of 1,246 per brood.

Sixty mixed broods, in which the sex of each individual was carefully determined, are listed in Table II, arranged in the order of percentage of males, from the highest to the lowest. In Table III are listed fifty additional broods, in which the percentage of males is based on an examination of 500 individuals in each brood. Reference to these tables will show that the percentage of males varies from 72.07 to 0.06, and further, that in over 58% of the broods less than 10% of the individuals in any given brood are males, while in 35% of the brood there is less than 3% of males. The most striking feature of these data is the fact that females are so frequently in excess of males. The significance of this will be discussed in a later section of the paper.

TABLE II. *Mixed Broods of Paracopidosomopsis.*

Brood.	No. of individuals.	Females	Males.	% of males.
1	974	272	702	72.07
2	1,289	400	889	68.97
3	1,237	482	755	61.02
4	636	294	342	53.78
5	828	385	443	53.50
6	545	272	273	50.09
7	1,162	595	565	48.71
8	848	529	319	37.62
9	1,512	1,009	503	33.27
10	1,847	1,287	560	30.32
11	839	586	253	30.15
12	943	701	242	25.66
13	998	746	252	25.25
14	1,706	1,353	353	20.69
15	628	509	119	18.95
16	662	542	120	18.12
17	1,022	878	144	14.09
18	1,364	1,195	169	12.39
19	1,797	1,584	213	11.85
20	1,041	923	118	11.35
21	1,477	1,357	120	8.12
22	1,115	1,033	82	7.35
23	1,307	1,211	96	7.35
24	1,303	1,199	104	7.21
25	1,196	1,122	74	6.19
26	803	754	49	6.10
27	1,432	1,352	80	5.59
28	868	822	46	5.30
29	1,423	1,349	74	5.20
30	1,050	996	54	5.14
31	1,883	1,791	92	4.89
32	1,259	1,203	56	4.45
33	1,457	1,394	63	4.32
34	855	828	27	3.16
35	1,046	1,015	31	2.96
36	1,124	1,091	33	2.94
37	1,000	972	28	2.80
38	757	739	18	2.38
39	847	827	20	2.36
40	1,301	1,271	30	2.31
41	1,209	1,183	26	2.15
42	1,260	1,233	27	2.14
43	1,682	1,647	35	2.08
44	1,592	1,559	33	2.07
45	1,258	1,232	26	2.07
46	692	679	13	1.88
47	1,220	1,209	20	1.64
48	1,115	1,097	18	1.61
49	1,036	1,021	15	1.46
50	1,798	1,774	24	1.35
51	1,075	1,061	14	1.30
52	1,475	1,456	19	1.29
53	1,349	1,336	13	0.96
54	2,003	1,986	17	0.85
55	1,017	1,010	7	0.69
56	1,165	1,159	6	0.52
57	1,143	1,141	2	0.18
58	1,493	1,491	2	0.13
59	912	911	1	0.12
60	1,550	1,549	1	0.06

SEX RATIOS IN PLATYGASTER RUBI

I have reared 105 broods of *Platygaster*, 67 from the carcasses of *Rhopalomyia* and 38 from those of *Walshomyia*. These are listed in Tables IV and V. The total number of individuals is 1,616, or an average per brood of over 15. The broods from *Rhopalomyia* are distinctly larger than those from *Walshomyia*, the former averaging slightly over 18, the latter only 10.5.

This difference is probably due to the fact that the larva of *Rhopalomyia* is almost twice as large as that of the second host, and thus furnishes a better supply of food for the multiplication of embryos. The smallest brood found contained five individuals (Brood 68), the largest thirty-seven (Brood 21).

TABLE III.-*Mixed Broods of Paracopidosomopsis*

[Per cent of Males Based on a Study of 500 Individuals]

Brood.	No. of individuals.	% of males.
61	2,028	67.20
62	1,043	47.50
63	1,149	44.00
64	1,540	43.90
65	1,463	42.80
66	1,134	37.00
67	1,397	31.60
68	940	31.20
69	1,123	29.60
70	996	29.00
71	1,045	26.00
72	1,467	24.80
73	1,822	24.00
74	1,233	22.60
75	1,685	21.80
76	1,095	19.60
77	1,745	19.00
78	1,201	18.00
79	1,301	16.00
80	841	14.60
81	1,410	14.60
82	1,605	14.20
83	867	13.60
84	1,347	12.20
85	1,167	11.80
86	701	11.00
87	1,403	9.20
88	1,602	8.20
89	1,606	8.09
90	1,228	8.00
91	1,614	7.40
92	1,675	7.40
93	1,258	6.50
94	1,139	6.00
95	841	5.20
96	1,111	4.40
97	1,665	3.40
98	1,351	2.80
99	1,633	2.80
100	1,857	2.40
101	794	2.20
102	1,306	2.20
103	1,092	2.00
104	980	2.00
105	1,005	1.80
106	1,334	1.20
107	1,550	1.20
108	1,560	1 00
109	1,469	0.60
110	1,227	0.60

In many respects the data collected on *Platygaster* are the most interesting available for any polyembryonic species. So far not a single male brood has been found. Perhaps a larger collection of data would reveal such broods. The scarcity or absence of pure male broods can be explained as follows: The car-

TABLE IV.—*Broods of Platygaster from Rhopalomyia.*

Brood	No. of individuals	Females	Males	% of males
1	7	5	2	28.59
2	36	26	10	27.77
3	34	26	8	23.52
4	26	20	6	23.07
5	13	10	3	23.07
6	31	24	7	22.58
7	31	24	7	22.58
8	27	21	6	22.22
9	9	7	2	22.22
10	14	11	3	21.42
11	34	27	7	20.58
12	20	16	4	20.00
13	15	12	3	20.00
14	26	21	5	19.23
15	16	13	4	18.75
16	11	9	2	18.18
17	11	9	2	18.18
18	29	24	5	17.24
19	36	30	6	16.66
20	24	20	4	16.66
21	37	31	6	16.21
22	19	16	3	15.79
23	26	22	4	15.38
24	13	11	2	15.38
25	13	11	2	15.38
26	20	17	3	15.00
27	14	12	2	14.28
28	7	6	1	14.28
29	23	20	3	13.04
30	23	20	3	13.04
31	15	13	2	13.33
32	15	13	2	13.33
33	16	14	2	12.50
34	16	14	2	12.50
35	17	15	2	11.76
36	26	23	3	11.53
37	9	8	1	11.11
38	28	25	3	10.71
39	11	10	1	9.09
40	11	10	1	9.09
41	12	11	1	8.33
42	12	11	1	8.33
43	12	11	1	8.33
44	13	12	1	7.69
45	13	12	1	7.69
46	13	12	1	7.69
47	13	12	1	7.69
48	15	14	1	6.66
49	15	14	1	6.66
50	15	14	1	6.66
51	15	14	1	6.66
52	15	14	1	6.66
53	16	15	1	6.25
54	16	15	1	6.25
55	16	15	1	6.25
56	17	16	1	5.88
57	18	17	1	5.55
58	18	17	1	5.55
59	18	17	1	5.55
60	18	17	1	5.55
61	22	21	1	4.54
62	9	9	0	0
63	13	13	0	0
64	14	14	0	0
65	15	15	0	0
66	16	16	0	0
67	19	19	0	0

cass of the host which contains the parasitic pupae is formed in the chamber of the gall, and the parasites upon emerging are confined within the gall for some hours before they are able to gnaw an exit hole through the wall. In all of these polyembryonic hymenoptera copulation takes place immediately after the insects emerge, a single male often mating with many females. Since practically every brood has at least one male present, it follows that unfertilized females rarely escape into nature. It is known from cytological and experimental studies that eggs laid by virgin females produce only male broods. Under these conditions, male broods are not to be expected in *Platygaster*.

A second point of interest is the scarcity of pure female broods. Only six have been observed, and all of these were reared from the carcasses of *Rhopalomyia* (Broods 62 to 67). However, the chief interest in these data is to be found in the character of the 99 mixed broods. Without exception the number of females in any brood exceeds the number of males. In fifty-three broods, or more than half the cases, there is but a single male present. Seventeen broods have two males each, and thirteen three each. The remaining broods have varying numbers of males, the highest having ten (Brood 2).

TABLE V.—*Broods of Platygaster from Walshomyia*

Brood	No. of individuals	Females	Males	% of males
68	5	3	2	40.00
69	10	7	3	30.00
70	11	8	3	27.27
71	8	6	2	25.00
72	5	4	1	20.00
73	18	15	3	16.66
74	12	10	2	16.66
75	23	19	4	16.95
76	19	16	3	15.78
77	13	11	2	15.38
78	7	6	1	14.28
79	7	6	1	14.28
80	15	13	2	13.33
81	8	7	1	12.50
82	8	7	1	12.50
83	8	7	1	12.50
84	9	8	1	11.11
85	9	8	1	11.11
86	9	8	1	11.11
87	9	8	1	11.11
88	10	9	1	10.00
89	10	9	1	10.00
90	10	9	1	10.00
91	10	9	1	10.00
92	10	9	1	10.00
93	10	9	1	10.00
94	10	9	1	10.00
95	10	9	1	10.00
96	10	9	1	10.00
97	10	9	1	10.00
98	10	9	1	10.00
99	10	9	1	10.00
100	11	10	1	9.09
101	11	10	1	9.09
102	11	10	1	9.09
103	11	10	1	9.09
104	11	10	1	9.09
105	11	10	1	9.09

SIGNIFICANCE OF THE SEX DATA

In the light of these data, how is one to explain the origin of mixed broods in polyembryonic hymenoptera? The obvious explanation, and therefore the one offered by all the pioneer workers in the field, is what may be called the two-egg hypothesis. It is suggested that mixed broods have developed from two parasitic eggs, one laid by a fertilized and the other by a virgin female. Undoubtedly such dizygotic broods do exist. Several of the mixed broods listed in the tables can be adjusted easily to this hypothesis. It is to be expected that a host egg would occasionally be visited and parasitized by two such females. But there are at least two obstacles which stand in the way of the universal application of the two-egg hypothesis to all mixed broods.

I have elsewhere pointed out and discussed these difficulties, so that here they may be referred to briefly: (1) The individuals of a mixed brood emerge simultaneously; therefore, it is necessary to make the assumption that both parasitic eggs are always laid at the same time. (2) In the great majority of mixed broods females far outnumber males. This is strikingly so in the broods of *Platygaster* and *Paracopidosomopsis*. It must be assumed, then, that an unfertilized egg in the presence of a fertilized egg is inhibited from fully developing; because when laid alone in the host egg it produces a pure male brood, in most instances as large as a pure female brood. In the absence of experimental evidence for sexual hormonic action in insects, it is difficult to square the facts revealed by a study of mixed broods with the two-egg hypothesis.

POLYEMBRYONY IN PLANTS

Seeds of certain plants sometimes contain several embryos. The mango, the rose-apple (*Eugenia jambos*), and the citrus fruits are all known to possess this characteristic. Above are shown several plants arising from a single seed of the mango. It is presumed that one of the embryos develops from the fertilized egg-cell; the remainder are derived from the tissue of the nucellus by budding. These adventitious embryos produce plants whose fruit resembles that of the parent tree as closely as that of a grafted individual. By this means, in fact, numerous seedling races of mangos and citrus fruits are propagated in the tropics. (Fig. 5.)

Since we had been able to show experimentally that if the host egg is oviposited by a virgin female, the resulting brood of parasites is always male, but if parasitized by a fecundated female a mixed brood develops, the question was raised as to whether the female of *Paracopidosomopsis* deposits two eggs at one oviposition, and if so, whether the impregnated female (as does the queen bee) could lay unfertilized as well as fertilized eggs. If this were so, it might explain the appearance of numerous mixed broods in this species. These questions have been tested out by appropriate experiments, and it is found that in two times out of three, or in about 66% of the cases, the female parasite lays two eggs in the host egg at a single oviposition. If it is assumed that in every case one of the two eggs is fertilized and the other is not, there is still a discrepancy of over 20%, because 87% of all broods in this species are mixed. Furthermore, I have studied, by means of smears, the two eggs laid at one oviposition by the fecundated female, and find that in the majority of cases both eggs are fertilized. In a few eggs it is difficult to demonstrate the presence of the sperm, although a failure to do so does not necessarily indicate that such eggs are unfertilized; because the position of the egg on the slide may be such as to obscure the sperm.

In this connection the conditions in *Platygaster* are very interesting. The host egg for this species is too small for smear preparations, so that it is necessary to resort to sections in order to determine whether the parasite deposits one or two eggs. The host eggs, which are usually laid in masses, were exposed to a mixed brood of parasites, and sections then made of these masses. Some of the eggs will, of course, be parasitized by more than one female, but the position of the eggs in the yolk of the host egg will show whether there has been more than one oviposition. In a group of 28 eggs the following data were obtained: Each of 14 eggs contains one parasitic egg; each of 11 contains two; and each of 3 contains three. In eight cases out of the eleven the two eggs are far enough apart to indicate two ovipositions, while in three cases they are close together, suggesting that the two eggs may have been laid at one oviposition. In each of the three remaining eggs the three parasitic eggs are not close together.[3] In view of these facts it is impossible to explain the origin of mixed broods of *Platygaster,* tabulated in Tables IV and V, on the two-egg theory.

The suggestion has been made (Patterson '15, '17*a*) that the appearance of so many mixed broods in some of the species of polyembryonic parasites may be accounted for on a monozygotic basis, namely, that a fertilized egg gives rise to males as well as females. This result could be brought about through the abnormal behavior of the two sex chromosomes during the early cleavage divisions of the egg. There are several different ways in which this might occur, but the most probable way is by the means of somatic non-disjunction (Bridges, '16). If this process should occur, certain blastomeres receiving but a single $\times$ chromosome would become the progenitors of male embryos. The sex ratios in the broods of Polygnotus in particular are more easily accounted for on this basis than by the two-egg hypothesis. This is especially true with reference to the large number of broods which show but a single male. The regularity with which certain sex ratios appear in many broods (*e. g.*, 9 females and 1 male. Broods 88 to 99) suggests that a single male is formed at some very definite point in the early development of the eggs. If somatic non-disjunction does occur during the early cleavage stages, it might be regarded as a very primitive type of paedogenetic development, in which some of the early blastomeres, functioning as egg cells, produce abnormal behavior of the sex chromosomes in the cleavage division.

[3] For these data I am indebted to Mrs. Lelia T. Porter, who is studying the development and cytology of these insects.

A non-disjunctional division in any of the first few blastomeres will necessarily result in the segregation of the sex factors (producing male and female), provided the cell in which it occurs has not reached the point beyond which it becomes the progenitor of a single embryo. If a blastomere has once passed beyond this point in its development, a non-disjunctional division in any of its descendants should result in the formation of a gynandromorph or mosaic, as several writers have interpreted the origin of gynandromorphs for the monembryonic egg. Evidence that gynandromorphs are produced in polyembryonic parasites is furnished by two cases which have come under my observation. In 1917, while studying a brood of *Paracopidosomopsis*, I found an individual in which one-half of the abdomen is male and the other half female. This spring I found a specimen in *Platygaster* which is clearly a gynandromorph. Sexual dimorphism in this species is very indistinct. The only clear mark of distinction is the antennae, which are somewhat stouter in the female than in the male, and, furthermore, the fourth antennal joint in the male is enlarged. The specimen in question has a male antenna on the left side of the head, and a female antenna on the right.

The final solution of the question concerning the origin of mixed broods must be found through cytological studies. However, one may attempt to interpret the data presented in this paper, the facts at any rate are perfectly clear and can be verified by anyone who will make a careful study of these insects.

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Estimating Intelligence

There is a general belief that it is a relatively easy problem to estimate a person's intelligence by looking at him; and teachers, physicians and employers are often compelled to make judgments as to the intelligence of a given person with no more data than can be obtained from a rapid survey of his appearance; hence such phrases as "he looks bright" or "he looks stupid." Even in the law courts rough estimations of intelligence are sometimes required. In the *Psychological Review* (Vol. xxv, No. 4) Mr. R. Pinter gives the results of an investigation he made for the purpose of testing the trustworthiness of these judgments. The author chose twelve photographs of children varying in intelligence from proved feeble-mindedness to unusually great ability, and asked groups of people to arrange the photographs in order of merit for intelligence. His groups consisted of physicians, psychologists, teachers and miscellaneous people. He found that the group of psychologists was the most nearly correct, but that on the judgment of no one group or of no one person could any reliance be placed. Several observers were consciously influenced by children of their acquaintance whom a photograph happened to resemble, and irrelevant trivialities quite frequently biased the observer's judgment. The author concludes that, although perhaps a living person would be easier to judge than a photograph, nevertheless these haphazard judgments are too untrustworthy to be of practical value; and that, whether the observer be a teacher, physician, or employer, it would be better to use objective standards, and he recommends that the use of mental tests should be considerably extended.—*Nature*.

WHY SHOULD NOT CEMETERIES PRESERVE OUR LIFE RECORDS?

A Plea That Cemeteries Should Become the Historic Spots Where the Records of Our Deeds and Our Likenesses Are Preserved

DAVID AND MARIAN FAIRCHILD

WHY should not our cemeteries be something more than the mere resting places of our bones? The savage tribes of Borneo bury their dead with quite as much ceremony as that with which we escort the remains of those dearest to us to the grave. They erect sticks over these burial places, and they soon forget them, just as we do.

Who has not hunted in vain through a great cemetery for some granite shaft that looks almost like scores of others, but which is precious because it witnesses the spot where some earthly form loved when alive disappeared forever into the earth from which it came. Scattered in more or less orderly groups over a hillside lie these spots, each sacred to some one, each visited perhaps on birthdays or marriage days by the living souls in whose memory the image that lies buried is slowly fading.

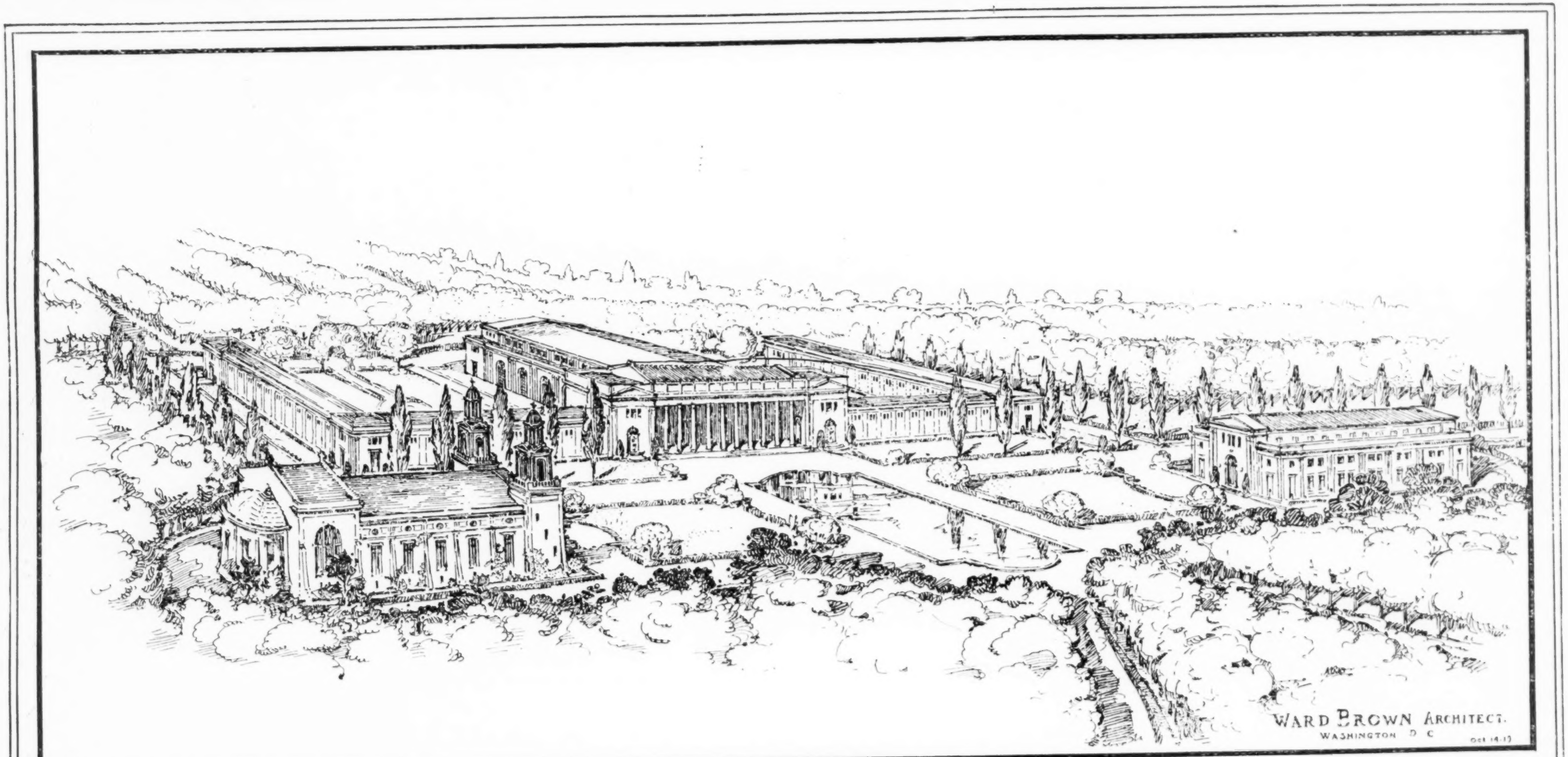
Sad places are these which mark man's disappearance beneath the sod, but, sad as they are, why should they be so hopelessly useless? Our dead are buried in them, and their bones are protected from molestation perhaps for centuries, but is that not really all? In a hundred years, a sinking gravestone, a name and date that are hard to read, and perhaps an epitaph—though now these are few—are the most we can get from any cemetery.

Yet these are the most sacred spots of the human race. Every career ends in one; that of the greatest human being who has lived as well as the smallest finds its close somewhere in a graveyard.

This has been for centuries, and is today, the attitude of our minds towards cemeteries, but has not the time come when a different point of view regarding them is possible? The theories of descent and of the continuity of the germ plasm have brought the realization that the living descendants of the dead are imperishable parts of them which go on living on this earth, and are an extension, a projection, of the life which shone out through the eyes and found expression in the voices of forms which lie buried beneath the cemetery sod. To know, to understand those who are living, we must know about the dead who bore them. It is not only the very few distinguished members of a family who leave their mark upon their descendents; we are just as much influenced by those who live their lives quietly and having done nothing spectacular, pass away without leaving any account of themselves in literature or art or business.

This growing conviction that we cannot understand people unless we know their ancestry is making it more and more necessary that the records of our ancestors be kept, but where? The most sacred spot appears to be that beneath which they are buried, but, as the graveyard is organized today, this spot reveals nothing whatever—teaches nothing. It is the spot of all most terrible to visit, for it is there that we gaze into the abyss of death—and wonder. Need this be so? Need our graveyards be, so far as teaching is concerned, as silent as the barbaric graveyards of the savage tribes? Can we not arrange to have them instead storehouses of all possible information about the dead who are in them?

The Pharaohs of Egypt thought to live on into eternity, and day by day



A PLACE FOR THE PRICELESS FAMILY RECORDS

"Let us picture in our imaginations such a graveyard as could be made, providing someone who could do it were convinced of its value. Somewhere in it there would be a beautiful building—as beautiful as art can make it—and in it a series of halls with alcoves and quiet places like a great library. Instead of books, however, there would be stored away in fireproof vaults the priceless records of the men and women—the families—who lie buried in the cemetery, and at any time these would be available to any one who had a right to them . . . . There would be photographs and accounts . . . . so that we could get an actual, tangible idea of the family. It would help us realize our place in the family and our responsibility to it." (Fig. 6.)

built their tombs more deeply into the solid rock, inscribing on the walls their deeds. They left accounts of themselves which thousands of years later have been deciphered and have revealed their lives. What they did five thousand years ago, and for a different reason and at great expense, the poorest of us now might do, leaving our images in the silver, of photographic prints, and accounts of what we did and who we were to guide mankind in its long studies of that great science of inheritance. What it required hundreds of slaves to accomplish in the days of the Pharaohs, the printing press and the camera can do for us all.

This record of life is made for many of us, but the accounts are scattered after we are dead, and our graveyards, instead of being the places where our deeds are recorded and where those imperishable shadows, our photographs, may be seen, are cold, dreary, silent and speechless.

We cannot help feeling that those who control our cemeteries have an opportunity here to exert a truly great influence for the betterment of our race. If they should gather for all who are buried in their confines all the data available at the time of the person's death, including photographs, and store this data as our libraries store books, having it in shape for easy display, would not such places become the centers of information regarding the stocks from which we came? A system of exchange could easily be worked out which would make it possible to trace the heredity of all.

There comes to most of us at some time the desire to see where our ancestors are buried, and we go to some New England village cemetery, or perhaps to some little churchyard abroad and succeed in finding the gravestones. Perhaps in the church warden's registry or the town clerk's office we find recorded the birth or marriage or death

"Sad places are these which mark man's disappearance beneath the sod, but sad as they are, why should they be so hopelessly useless? . . . In a hundred years, a sinking gravestone, a name and date that are hard to read, and perhaps an epitaph—though now these are few—are the most we can get from any cemetery." (Fig. 7.)

of some one of our name. What a satisfaction it would be if instead we could go into some quiet room and have brought to us to look at, to hold in our hands and to show to our children the actual photographs of our forefathers, the accounts of what they had done, little incidents of their lives, anything which gives them a real personality for us and strengthens our interest in the family.

Let us picture in our imaginations such a graveyard as could be made, providing someone who could do it were convinced of its value. Somewhere in it there would be a beautiful building —as beautiful as art can make it—and in it a series of halls with alcoves and quiet places like a great library. Instead of books, however, there would be stored away in fireproof vaults the priceless records of the men and women —the families—who lie buried in the cemetery and at any time these would be available to any one who had a right to them and they could be taken in to one of the rooms or alcoves to be looked over and studied. There would be quiet reading rooms and special viewing contrivances where moving pictures of families could be seen and places fitted up so that phonographic records of the voices of the past could be heard. There would be photographs and accounts also of the uncles and aunts, and of the little children who died young, so that we could get an actual tangible idea of the family. It would help us realize our place in the family and our responsibility to it.

There need be nothing dreary or morbid about such a place, indeed if it were so the whole object would be defeated. It should be the natural place to which one would take visiting relatives and those who were coming into the family by marriage, and the children would go too and realize that it means something to belong to a family, that it is a pleasant thing to be proud of one's family and that one would like to add to the family prestige oneself.

Visits such as these would give a background to our lives—would tie us to the past and also make us realize that to our children must come the burden and the pleasure of passing on the light of our lives into that great and wonderful future *upon this earth* in which increasing millions of babies still unborn must live their lives, let us hope, as happily as *we* have.

At funerals and at weddings families gather together, but we part again too often without strengthening the ties which would bind us to the past and make us feel that pride of family which must be at the bottom of the making of a better race. First, better families, then their spread and increase, and through them a gradual raising of the level of the whole race. It is the only way unless the laws of heredity are a myth.

To lay up treasure for ourselves in heaven is one thing, but is it not quite as noble to try to do things now which will make the earthly lives of future generations happier and more spiritual and more wonderful?

If we are to make the world better; if that is one of the reasons for our existence, why is not the establishment of these Libraries of Family History, a really constructive way of doing it? Why does not someone do it?

A SUPPOSED SHEEP-GOAT HYBRID

A Remarkable Skin Secured by a Trader from the Navajo Indians of Arizona

FIGURE 8 shows a very remarkable skin which has been received from Mr. J. W. Bush, of Castle Butte, Arizona. This skin seems to partake of the characters of both the sheep and goat.

The color is a mixture of light brown and white on the back, sharply distinct from a very dark brown color on the belly. The legs are largely dark brown with, however, a streak of light brown in front. Except for the irregular areas of white on the back, the color and pattern closely resemble the Barbados sheep. A similar pattern occasionally appears among Rambouillets, as in a case described by L. L. Heller in THE JOURNAL OF HEREDITY, vol. vi, No. 10 (October 1915).

The sheep of Arizona are very much mixed in color. According to an estimate, 15 to 20% are dark colored. The color of the skin thus might well be that of a sheep or sheep hybrid. The brown color is also, of course, common among goats.

The coat is largely composed of short coarse hair. In many places there is a soft, woolly undercoat. In small patches on the shoulders, along the back and on the belly, this wool has accumulated in tangled mats. On the hind quarters the wool becomes more hair-like, but is much finer than the hair in the same part of the coat, as well as much longer, reaching a length of four or five inches. The tail is entirely covered with fine hair or coarse wool of this character. It is somewhat coarser than Lincoln wool. Below is Mr. Bush's account:

"Under separate cover I am sending you a hybrid goat-sheep skin.

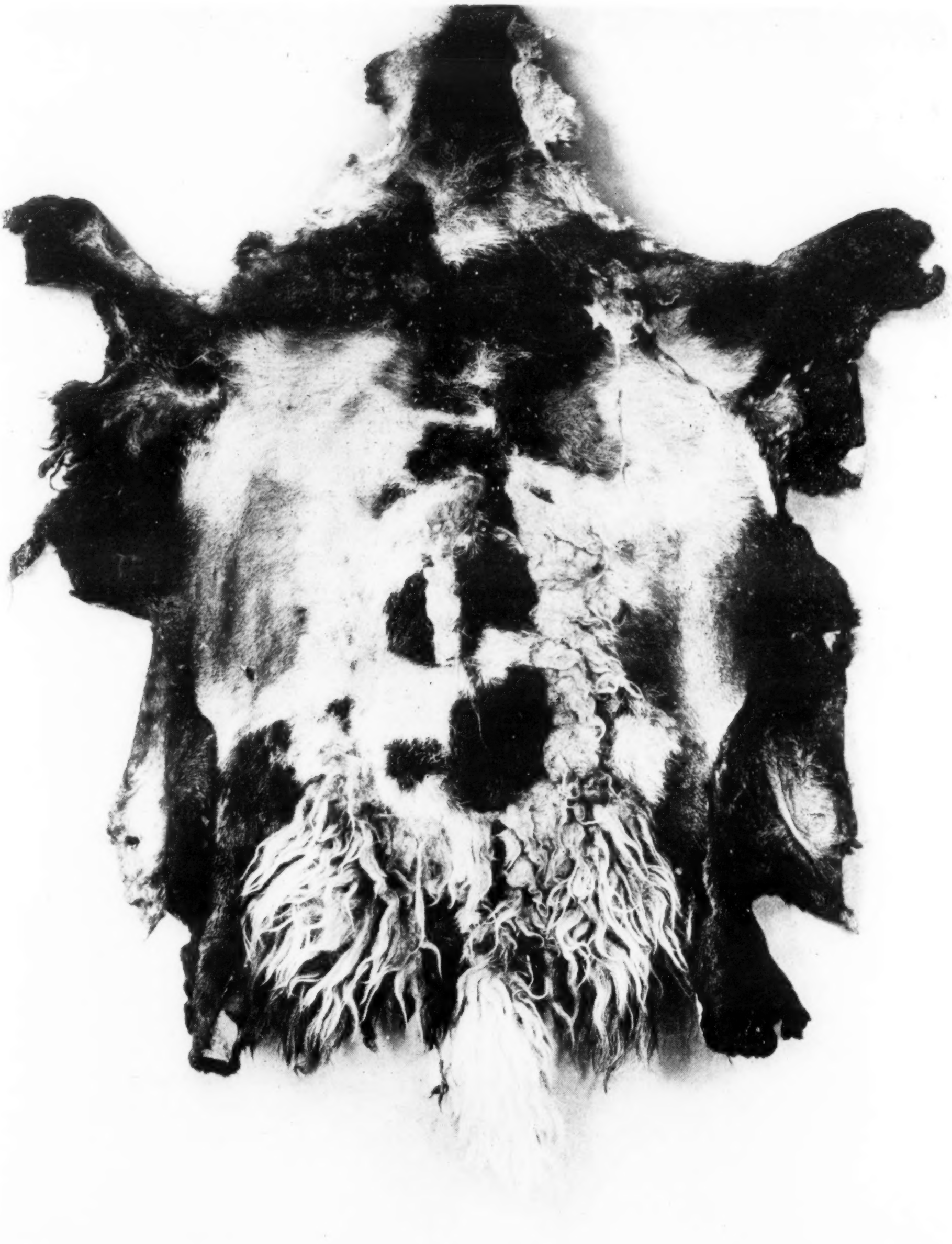
"The skin was presented to me more than a year ago by Mr. E. J. Marty, who is engaged as trader with the Navajo Indians at Indian Wells, Arizona. . . .

"This is the result of a cross between a native (Mexican) goat and a sheep, the original owner of which I have been unable to locate, as the skin was sold by some Indian without special notice taken of it at the time.

"I have shown it to many of our oldest Indians and all say they have never seen anything like it before."

In view of the rather unsatisfactory history, it can hardly be definitely affirmed that this is a sheep-goat hybrid. Yet it seems unlikely that the patches of wool could come from a goat, nor could the coarse hair coat come from a sheep. Sheep-goat hybrids are certainly not common, and it has often been denied that they can be produced at all. They are occasionally reported, however. One such case was reported by W. E. Spillman in the *American Breeders' Magazine*, vol. iv, No. 1 (1913).

It is to be hoped that this question of the occurrence of sheep-goat hybrids will be settled by experiments which leave no doubt in regard to parentage.



SKIN OF THE SUPPOSED SHEEP-GOAT HYBRID

The color is a mixture of light brown and white on the back, sharply distinct from a very dark brown color on the belly. The pattern closely resembles that of the Barbados sheep. (Fig. 8.)

DETAILED VIEW OF SKIN SHOWN IN FIG. 8

The coat is largely composed of short, coarse hair but in many places there is a soft, wooly undercoat. In small patches this wool has accumulated in tangled mats. On the hind quarters the wool becomes more hair-like. (Fig. 9.)

DEMOCRACY AND HEREDITY—A REPLY

ALLEYNE IRELAND

GOVERNMENT, as an art and a science, is the only mundane subject which, from a period antedating the Christian Era, has engaged the earnest attention alike of philosophers and men of affairs. But at no time in recorded history has government been a matter of personal concern to so large a proportion of the world's inhabitants as it is today. It is in the domain of government alone that all the secular interests of the rich and the poor, of the strong and the weak, of the wise and the foolish, of the young and the old, of the industrious and the idle, of the sick and the hale can be marshalled in a single category.

In relation to the problems created by the war the broad question of government assumes, therefore, an importance which the general public, even in face of its preoccupation with immediate economic and industrial difficulties, is beginning to recognize.

To a lifelong student of government, the signing of the armistice in November, 1918, with its promise of an unprecedented strain upon all national administrations, appeared to mark the arrival of a moment when thoughtful people would welcome a discussion of government which aimed rather to explain general phenomena than to attack or defend particular administrative projects. Accordingly I sent to the JOURNAL OF HEREDITY an article entitled "Democracy and the Accepted Facts of Heredity," which was printed in the issue for December, 1918, and was discussed in the May and June issues of 1919 by Prof. Edwin G. Conklin, of Princeton University; Mr. Madison Grant, Mr. Prescott F. Hall, Prof. O. F. Cook, and Mr. Robert Carter Cook.

It will serve the convenience of all readers of this article if I compress into a few paragraphs the gist of my first article. I stated, in effect:

1. That my observation of government in a score of countries had convinced me that, with few exceptions, the best governed countries were those in which the mass of the people had the least control over the administration of public affairs.

2. That in all worldly activities, save only those linked with politics, expert knowledge and firm leadership are universally recognized as the sources of success.

3. That in political matters we adopt the opposite principle, namely, the control of the expert by the inexpert.

4. That this amounts in practice to the substitution of a quantitative for a qualitative foundation for authority.

5. That the worse become the consequences of applying this principle the louder do we call for its wider application.

6. That this is the expression of a rhapsodical or irrational attitude toward the democratic form of government, in conformity with which we attach more importance to the form than to the results of government, and refuse to draw the only logical conclusion when these results are, over a long period and over a very wide area, patently and increasingly unsatisfactory.

7. That my dissent from the conventional view of democracy (as we practice it) as a sound, political principle was based upon four main considerations:

(*a*) That the individual and not the mass has been the main source of human advancement.

(*b*) That mental and moral traits in the individual are derived chiefly from heredity and not from environment.

(*c*) That acquired characteristics are not inheritable.

(*d*) That the general mating—through the pressure of social propinquity—of the more intelligent with the more intelligent, and of the less with the less, tends to make talented strains more talented and to depress other strains further and further below the line of mediocrity.

The central idea of my article was that, if my biological statements were true, the conclusion was inescapable that efficiency in government could not arise from or be made to depend upon its democratic quality, the word "democratic" being employed by me, of course, to express democratic experience during the past twenty centuries, and not democratic hopes for the next twenty.

Of the four articles which discuss the views here advanced, two are written by men whose professional work has made them specially familiar with biology, and two by men who, from similar causes, are specially familiar with affairs. The biologists endorse the greater part of my biological statements, whilst strongly dissenting from my application of them, and the men of affairs endorse the greater part of my statements on government. The satisfaction with which I contemplate this situation may be measured by considering the position I would have been in if the principal support for my views on government had come from the biologists, and for my views on biology from the men of affairs.

I do not suggest that a biologist may not have studied government to good purpose, or a man of affairs biology; and, in common with all readers of the JOURNAL, I am aware that Mr. Madison Grant, the lawyer, enjoys an international reputation as an anthropologist, and that Professor Cook, the botanist, is well known as an authority on African colonization.

Before taking up those points on which the discussion has disclosed marked differences of opinion it is advisable to refer to one on which there seems to be substantial agreement, namely, the extremely unsatisfactory state of American democracy after nearly a century and a half of free operation under conditions more favorable in most respects than those under which any other democracy has ever functioned.

As Professor Conklin opens his reply to me with a reference to his article on "Biology and Democracy" in the April, 1919, issue of *Scribner's Magazine,* I may quote from it his description of present-day democracy in this country:

"Our lack of specialization is reflected in our contempt for specialists and experts of every sort. The belief is widespread that one man's opinion is as good as another's and that expert knowledge is merely another way of fooling the people. We intrust education to those who can find no other occupation, apparently with the idea that anyone can teach. We leave the control of food, fuel, clothing, and other necessaries of life to speculators and middlemen, and the health, happiness, and employment of the people to Providence or to selfish exploiters. . . . We elect demagogues and grafters to political office so frequently that the very name 'politician' has come to be a reproach. We send narrow partisans to Congress, and, by stupid adherence to party regularity, men wholly untrained in statesmanship are frequently put into the most important public places."

After reading the foregoing description of American democracy as it appeared to Professor Conklin's *observation* in April, 1919, it was not without astonishment that I read his *judgment* of American democracy as expressed, also in April, 1919, in the JOURNAL OF HEREDITY, where he concludes his reply to me with these words: "After all the merits of any system of government should be measured by its actual results on society as a whole, over long periods of time, and measured in this way democracy has no cause as yet to be fearful of the results."

If the results to date of American democratic government, as Professor Conklin describes them, are no cause for fear; it is not easy to imagine upon what kind of results a just apprehension might be founded.

I do not know to what extent Prof. O. F. Cook and Mr. Robert Carter Cook find themselves in agreement with Professor Conklin's scathing indictment of the present state of American democracy; but they have, in their article in the JOURNAL for June, 1919, launched an indictment of their own against democracy as it is, which, though much shorter than Professor Conklin's, is more comprehensive and more formidable. They say: "The need is to give special ability or usefulness a selective value, to preserve and increase the family stock, but our system works generally in the opposite direction of using up and exterminating talent as rapidly as possible," and, in another connection, "our tendency is to restrict ourselves further and further toward mediocrity and inferiority."

The authors of these observations reach a conclusion not less astonishing than that reached by Professor Conklin from his observations, namely, "Our experiment in democracy is very different from any that preceded it, in being aimed toward scientific government."

It is not incumbent upon me, I am glad to say, to reconcile this statement with Professor Conklin's statement about our contempt for specialists and experts of every sort, or with his declaration that "almost every citizen thinks that he could solve complex problems of government ranging all the way from international relations to parochial affairs better than those who have devoted years of study to them."

I may dismiss this phase of the discussion by asking the reader to judge in what measure the observations of Professor Conklin, of Professor Cook, and of Mr. Robert Carter Cook, when considered in relation to the judgments they found on them, justify the following paragraph in my first article:

"Those who assume the task of reconciling the facts of democratic control with its theory adopt an expedient which places the whole issue beyond the reach of reason. They lay down the rule that democracy must not be judged by its yesterday or by its today, but by its tomorrow, and that so fast as tomorrows become yesterdays even so fast must all adverse evidence be discarded as worthless. Just below the ever-receding horizon of time there lies, almost in sight of those who accept this rule, the pleasant land where education and dietetics shall have made the majority of mankind into political units from which there can be built up a government of benevolence, righteousness, and efficiency."

It is when my critics, my supporters, and myself seek to account for the existence of those evils which we unite in deploring that we separate into two camps, one affirming that the causes lie fundamentally in the operation of biological law, the other that they do not, the negative position being taken by the biologists.

The real issue, when stripped of all dialectical trappings, is whether good government (however it may be defined) depends ultimately upon good human qualities or upon good political machinery. If it depends chiefly upon the former, all discussions of government must be founded in biology; if upon the latter, the discussion must center around constitutional law and political technique.

My own view is that since government forms are merely the instruments through which men administer their public affairs, the essence of government is to be sought not in the shape of the instrument—still less in its name—but in the character of those who employ it.

Viewed from this standpoint the distinguishing mark of modern speculations on government seem to me to be a constant effort to find in political machinery a substitute for human character, and a persistent determination to attribute all failure in government to any cause rather than to the widespread distribution of stupidity and corruption in man.

It is a most curious circumstance that Professor Conklin, Professor Cook and Mr. Robert Carter Cook should describe the present state of democracy in terms which imply a widespread dis-

tribution of stupidity and corruption; should agree that mental and moral qualities, such as stupidity and corruptness, are chiefly derived from heredity; should agree that acquired characteristics, such as may result from secular education and religious training, cannot be transmitted through heredity; and should also agree that there is no essential connection between biological law and the amount of stupidity and corruption in democratic government.

What is not less curious is that none of the dissentients from my hypothesis, that in a democracy like ours the character of the government depends chiefly on the character of the people, and that the character of the people is determined chiefly through the operation of biological law, states, in his reply to me, by what other causes, in his opinion, the quality of government is in fact determined. Indeed, so far as these gentlemen do assign causes for bad government—and they say very little on this point—they assign causes which are of a biological nature.

For instance, Professor Conklin says: "Many faults of democracies are not so much results of the form of government as of the condition and character of the people." But surely character is a biological factor. Again: "A democracy no less than an autocracy is a government by leaders, but in the former case these leaders are chosen by the people and are responsible to them and in the latter they are not. . . ." But surely, whether the leader be good or bad, whether he be elected by the people or appointed by a king, the qualities of his leadership are not conferred upon him by his appointment or election, but are derived from his mental and moral characteristics; and these, as most biologists, including Professor Conklin, agree, are derived chiefly from the operation of heredity; which brings us back again to biological causation.

Professor Cook and Mr. Robert Carter Cook complain that we do not give special ability and usefulness a selective value and that our tendency, is to "restrict ourselves further and further toward mediocrity and inferiority." But mediocrity and inferiority are admittedly derived chiefly from heredity; and the fact that ability and usefulness are not given a selective value under our democratic system can only be due to the ignorance and stupidity of the mediocre and inferior majority which exercises control by force of its numbers.

A logical consideration of these truths should, it seems to me, have led their authors to a conclusion closely similar to that expressed in my first article, namely, that the majority of people being mediocre or inferior, it is unreasonable to expect that majority to provide a government which will not be mediocre or inferior. But so far from reaching any such conclusion, they describe my inferences as "reactionary" and "archaic," compare me with "another disillusioned specialist in government," and, having for the purpose of destroying one part of my argument, stated their conviction that we are breeding toward mediocrity and inferiority, they reprove me for not "looking forward to a world of capable, right-minded people. . . ."

Now, as a matter of fact, my sincere hope, though hardly my confident expectation, is that we may some day see a world in which capable and right-minded people shall exercise a much greater power in government than they now do. This hope is founded on the opinion that the two extremes of capacity and incapacity, of right-mindedness and wrong-mindedness, are, through the operation of assortative mating, becoming more distinctly separated from each other in the social scale, and that the more clearly this becomes evident the more likelihood there will be that special ability and usefulness will be given a survival value in our political system.

This brings me to a discussion of the only purely biological point on which my critics and myself are not in agreement—the effects of assortative mating.

I was careful to say in my former article that this was a point upon which biologists alone were competent to ex-

press an authoritative opinion. I must still leave the determination of this question with the biologists; but Professor Conklin having called for the evidence upon which my tentative opinion was based, I am glad to furnish it.

My statement appeared to me to be justified by the results of two investigations into the social distribution of genius and talent in the United Kingdom. The first was made by Prof. Havelock Ellis; the second, which is in continuation of the first, by Dr. Frederick Adams Woods. The material worked over in each case was furnished by the "Dictionary of National Biography" and by the successive supplements to that work. The former investigation is embodied in Ellis's "Study of British Genius"; the latter is not yet in print.

What these investigations disclose is that over a period of several centuries there has occurred a striking and progressive decline in the cultural contribution from the "lower" classes, and, of course, a corresponding relative increase in the contribution from the rest of the population.

It appears that, from the earliest times to the end of the nineteenth century, the contribution to eminent achievement made by the sons of craftsmen, artisans, and unskilled laborers yielded 11.7 per cent of the total number of names utilized from the "Dictionary of National Biography"; that the representatives of that class who were born in the first quarter of the nineteenth century yielded 7.2 per cent of the names; and that those born during the second quarter of the nineteenth century yielded only 4.2 per cent of the total.

These figures are of great interest when they are considered in relation to the social and political history of England during the nineteenth century.

Everybody knows that in England the nineteenth century witnessed a rapid and all-pervading democratization of social and political conditions. It was during this century that the English parliamentary system became, for the first time in the six hundred years of its existence, an institution representative of the great mass of the people; that schooling was made available for all; that in industry, in politics, in society, the gates of opportunity were opened wide for any person of whatever parentage who could make any contribution in any field of achievement; that peers became business men, and business men peers; that any scientist, any scholar, any painter, sculptor, musician, poet, novelist, actor, dramatist, engineer, chemist, architect, shipbuilder, lawyer, or merchant, whose talents had made him prominent in his calling, could entertain a reasonable hope of finding wealth in the favor of the public and a title of nobility in the appreciation of the political leaders.

In England in the nineteenth century, then, there were to be observed, in a measure never before attained in any age in any country, the conditions which give every man a chance, according to the qualities of his mind and temperament. If social opportunity was less free than in the United States, political opportunity was very much greater—a fact which can be verified by anyone who will take the trouble to compare the number of professions and occupations represented in the House of Lords and in the House of Commons with the number represented in the Senate and the House of Representatives at any moment during the past fifty years.

With every circumstance of life growing constantly more favorable to the self-assertion of genius and talent in the "lower" classes in England, how was it that the contributions to eminent achievement from that group fell from an average of 11.7 per cent of the total to a proportion of 4.2 per cent?

It seems to me that as the vast improvement in environmental conditions had not only failed to produce an increase in high achievement by those whom this improvement had done most to serve, but had, on the contrary, taken place *parri passu* with a very serious decline in achievement, the cause must be sought in an influence strong enough to offset whatever beneficent effects im-

proved environment might actually exert upon a stationary class during a single generation.

This influence I deem to have been that of assortative mating. Its operation appears to have been of a dual character. On the one hand, the effect in heredity of intelligence mating with intelligence and of stupidity mating with stupidity—to put the matter roughly—has been to perpetuate and to increase these opposite traits in the respective groups. On the other hand, the practical social consequence of these effects being produced under conditions of an ever-widening democratization of life has been that the more intelligent and talented elements in the "lower" classes have constantly been rising out of their class into one socially above it, and that this movement is draining the "lower" classes of talent and genius, tion, is increasing the genius and talent of each succeeding upper layer in the and, through a process of social migrasocial series.

Now, if assortative mating really occurs, as an inevitable consequence of social propinquity, and if democratic opportunity does actually enable a family to move upwards in the social scale, those who question the soundness of my inferences from these facts are placed in a curious dilemma.

If they admit the existence of assortative mating as an ordinary, everyday occurrence, they must, it seems to me, either deny the part played by heredity in determining the character of offspring or renounce the view that gence and penalties for stupidity. If democratic conditions have any tendency to provide rewards for intelli- such rewards and penalties are in fact apportioned, there must of necessity exist a constant upward and downward genetic pressure tending to produce an increasing difference between the two ends of the social spectrum; if such rewards and penalties are not among the consequences which follow the application of democratic principles then, so far from democracy presenting to every man a better chance in life, it must have the effect of placing a premium on inefficiency and a handicap on talent.

Professor Cook appears to accept the latter alternative when he says: " . . . our tendency is to restrict ourselves further and further toward mediocrity and inferiority. . . . The need is to give special ability or usefulness a selective value, to preserve and increase the family stock, but our system works generally in the opposite direction of using up and exterminating talent as rapidly as possible."

I may point out that, viewed in its relation to the future of democratic government, this statement describes a condition much more unfavorable than that which would exist if truth were found to lie in my hypothesis of an increasing intelligence and ability at one end of the social scale and of an increasing stupidity and incapacity at the other. In the former circumstances there could be found no reasonable hope of improvement; in the latter there could be discerned at least the possibility that the balance of political power might some day be transferred from numbers to intelligence.

Professor Conklin appears to regard my general argument as leading to the conclusion that the world should, in my opinion, be ruled by a fixed hereditary class; and he refers to "a real movement against fixed hereditary classes."

To whatever extent this movement is effective it claims my hearty support; but my support is entirely dependent upon the appearance in the phrase of the word "fixed." Wherever an hereditary class has become fixed—as was notably the case in Austria—the consequences have been disastrous, and for a very simple reason, namely, that such a "fixed" class cannot draw into its genetic reservoir the best elements of other classes.

In my judgment, the separation of society into classes—a phenomenon to be observed in every society, whatever may be its cultural or political status—is not only inevitable under all circumstances, but is highly beneficial wherever there is assured a free social migration from class to class. As man is con-

stituted—and one has only to compare the satire of Aristophanes with that of Bernard Shaw to be convinced of the permanence of his general characteristics—no greater stimulus can be afforded to talent and industry in the individual than the certainty that their diligent application will keep in a higher social order those who are already there and will carry into a higher social order those who are born in a lower.

In conclusion I may refer to the unanimous dissent expressed by my biologist critics from my conviction that the best governed countries are those in which the mass of the people have the least control over the administration of public affairs.

I cannot do more in this place than somewhat extend my former statement of this conviction, for it will readily be appreciated that a full discussion of comparative government would not be acceptable to readers of THE JOURNAL OF HEREDITY, and would demand more space than the journal could possibly afford it.

My conviction, then, is based upon a somewhat extended observation of government in practice. I have observed the operation of constitutional democracy in England, in Canada, in the United States, in Australia, and in France; of constitutional monarchy of a non-democratic type in Germany; of an almost pure absolutism in the Kingdom of Sarawak, Borneo; of non-parliamentary paternalism in India, in Java, in Burma, in Indo-China, and in the Federated Malay States; of parliamentary paternalism in the Straits Settlements, in Barbados, in British Guiana, and in Hong Kong.

So confused is the terminology of government that no standard definition of terms is available. I have adopted the foregoing classification simply for the purpose of making clear a distinction which is, I believe, vital to an understanding of any question connected with comparative government.

So far as my observation goes, England, Canada, the United States, Australia, and France have, in varying degrees, been less well governed in modern times than the countries which follow them in the list. The reason for this is, in my judgment, very clear, namely, that in the former group local politics have cast a blight upon the administrative functions of government, whilst in the latter group almost all administrative projects have been conceived and carried out, chiefly by experts, on the basis of their intrinsic merits. In other words, in the latter group of countries the health, comfort, convenience, and prosperity of the people have not been the pawns in that political game so wittily described by Mr. Balfour as "a system of organized quarrelling."

A word of explanation is necessary in regard to Germany. Until 1914 we heard little but praise of Germany's internal administration. As Professor Cook and Mr. Robert Carter Cook say in their reply to me: "German cities were cleaner than English or American cities. The visitor to Germany did not see the docks of Hamburg lined with human scarecrows, or little children going barefoot in the snow, as in the streets of Liverpool. Disheveled slums like those of New York or London did not exist. There were no hungry beggars or aimless people wandering in rags. Irresponsible indigence was as strictly forbidden as other misdemeanors."

Now it was surely not any of these things which led Germany to adopt her foreign policy of violent aggression or to carry it out with such infamous barbarity. To attribute her conduct to such causes would be to declare that clean, healthy, well-educated, and industrious people are more affected than others toward lust and violence. It was because German *Administration* was controlled by a special class that it was so excellent; it was because her *Policy* was controlled by a special class that it was so pernicious.

The distinction between policy and administration, as the two elements in government, is seldom drawn by writers on politics; yet it is, in fact, mainly upon the drawing of this distinction that good government depends.

The comparative inefficiency of government in democratic countries is due in a large measure to the circumstance that a single method, that of the ballot-box, is employed to achieve two wholly different aims—the ascertainment of the popular will, and its execution.

So long as the task of government was little more than to build the post-roads, impound stray cattle, punish a few criminals and misdemeanants, and collect a few taxes, it was easy to discern the popular will; and to execute it required little more than common sense.

Today, however, there is practically nothing which is not the concern of government; and in relation to the vast majority of administrative problems the public is not only absolutely incompetent to form an opinion, but is almost equally incompetent to select someone who is competent to form one. Assuming, however, that there is an informed majority opinion in regard to any question of domestic policy; and assuming further—what is a very wild assumption—that this opinion upon this question is reflected at the polls; we still have to face the fact that the carrying out of the public will today is for the most part a matter of extremely technical knowledge, a matter of chemistry, of engineering, of bacteriology, of electricity, of preventive medicine, and of other highly specialized sciences.

It is this shift in the center of gravity of government which has escaped the notice of those who approach the question of politics as though patriotism and democracy were still chiefly concerned in emancipating the people from the tyranny of a king, and who seem to believe that the people will always be content to accept an impotent political freedom as a substitute for efficient administration.

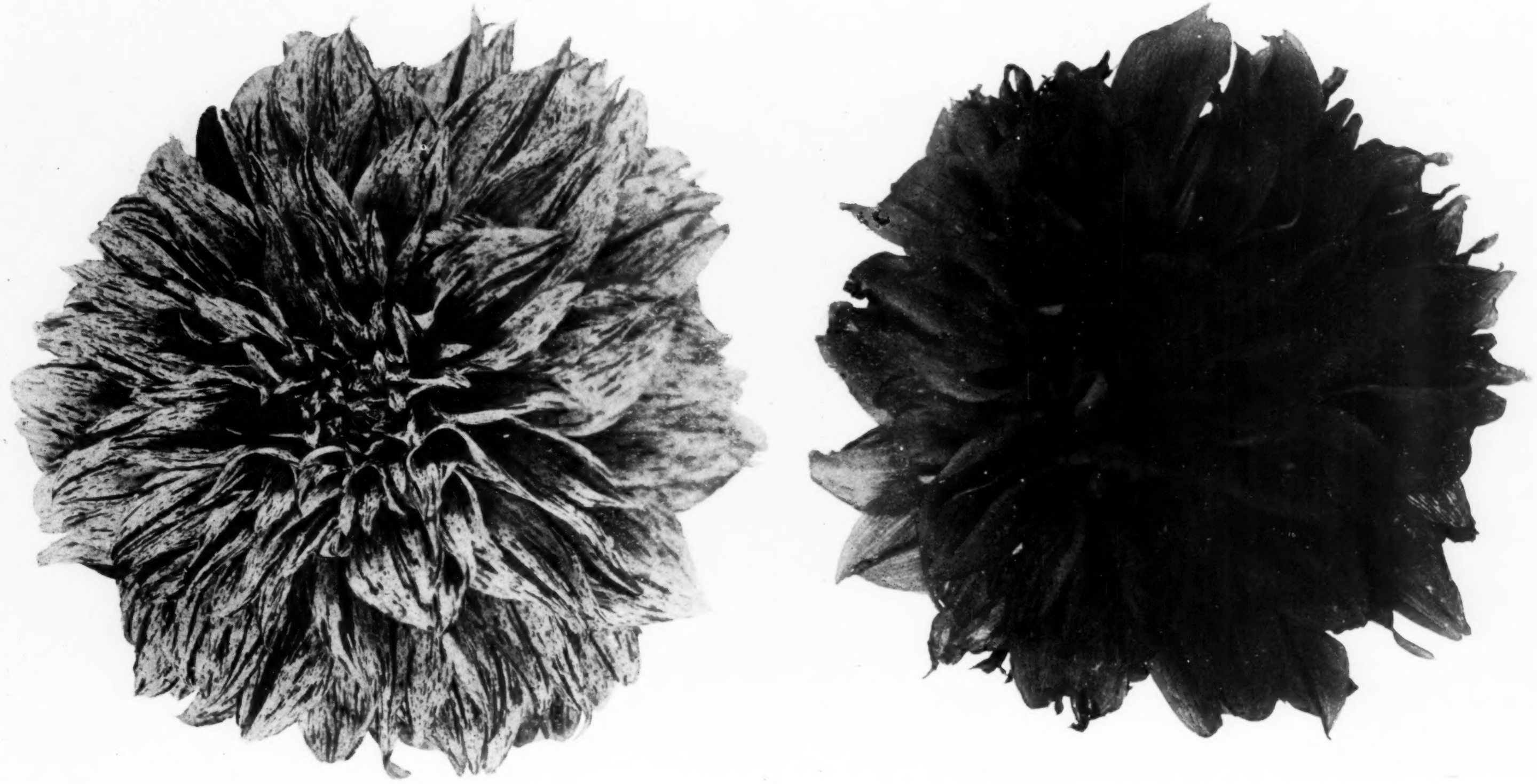
A BUD VARIATION OF THE LE GRANDE MANITOU DAHLIA

A. D. Shamel

In THE Journal of Heredity for December, 1918, the writer described a striking bud variation in a dahlia plant found in a garden at Riverside, Cal. Since that time he has been on the lookout for similar instances of dahlia bud sports. In September the dahlias of southern California are in full bloom. While visiting a citrus ranch at Alta Loma on September 9, 1919, my attention was called by the manager, Mr. W. M. Mertz, to a very interesting case of bud variation in the well-known dahlia variety Le Grande Manitou. The two plants of this variety are growing in the door-yard of the manager's house and are in fine physical condition. The typical flowers have a white ground and are striped and speckled with violet. The flower on the left in Fig. 10 is a representative bloom of the Le Grande Manitou from these plants.

The two Le Grande Manitou plants both produce, in addition to the typical flowers, blooms having solid violet color as shown in the flower on the right in Fig. 10. At the time of our examination of these plants one violet bloom was found on each plant to about twelve of the typical flowers of the variety. Mr. Mertz told the writer that up to this time each plant had produced several violet blooms.

As can be seen in the illustration, the two flowers, borne by the same plant, vary in other characteristics than that of color. The number, shape, arrangement and size of petals are decidedly different in the two blooms.



LE GRANDE MANITOU DAHLIA

The typical Le Grande Manitou flower is here shown on the left. On the right is a bud variation from the same plant. This is one of the striking cases of bud sports in dahlia plants and illustrates the origin of new varieties from bud variations. (Fig. 10.)

DEFICIENCY IN INTELLECT FOUND TO BE CORRELATED WITH DEFICIENCY IN THE NUMBER OF BRAIN CELLS

PATHOLOGISTS have known for some time that imbecility depends in part at least upon a deficiency in the actual number of cells of the higher cerebral regions. Hammarberg in 1895 showed by actual counting that such is the case. This theory is supplemented and supported by some recent work on the lesser-brain.

"In the spring of 1916, while examining the cerebellum of a general paralytic, the writer[1] was first impressed by the fact, familiar perhaps to most neuropathologists, that in this disease there is often a disintegration and disappearance of a large number of the Purkinje cells, leaving, however, the basket of fibers which normally surrounds them. Over a year later, while examining the cerebellum of a microcephalic idiot, the same scarcity of Purkinje cells was observed, with the difference, however, that the section did not show the same evidence of the cells having become reduced in number by disintegration; the empty pericellular baskets were not found as in the case of paresis; it seemed, rather, that through some defect of development the normal number had never been present.

"In order to get a fair basis for comparison, a number of cerebella were studied and the relative frequencies of cells noted. In some of the cases the cells appeared to be almost uniformly distributed and with few large spaces between them; others showed losses similar to the two cases already mentioned.

"Among the cerebella examined was one of a man who had died at about the age of sixty-five years after a protracted illness, and this, too, showed a distinct loss of cells. So from this preliminary set of observations it seemed clear that the number of Purkinje cells is variable under different conditions.

"It is well known that in paresis, in extreme old age, and in low grades of feeble-mindedness there is ordinarily a considerable degree of deficiency in motor coördination. The question consequently arose, how far is it possible to find differences in the number of cells that will account, partially at least, for the observed differences in behavior?

"The writer's primary interest at the time of taking up this investigation lay in the question of the anatomical basis of mental defect, and it seemed not improbable that a careful study of the Purkinje cells might throw some light on one of the most evident deficiencies found in such cases. The human motor mechanism is much more highly developed than that of lower forms, especially with reference to speech, hand movement, and the maintenance of equilibrium while standing or walking. Mental defectives generally show less motor control along these lines, and it is desirable that we know as far as possible the neural basis for such lack of coördination. A further reason for making a study of the cerebellum in such cases is found in the fact that a number of writers, especially Tredgold ('03) and Bolton ('03, '10) in England, have emphasized, perhaps unduly, the importance of the frontal lobe of the cerebral cortex as the area particularly affected in amentia. It accordingly seemed worth while to determine whether the brains of aments show defects in other parts, such as the cerebellum, which is not generally associated with intelligent reactions as such."

Dr. Ellis at the close of his article comes to the conclusion that in low grade mental defectives there is a distinct deficiency either numerically or cytologically in a large percentage of the Purkinje cells. He does not go into the question of heredity, but he does contend that practically all the cases are due to some form of antenatal degeneration.

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[1] Robert S. Ellis: "A preliminary quantitative study of the Purkinje cells in normal, subnormal and senescent human cerebella, with some notes on functional localization. *The Journal of Comparative Neurology*, vol. 30, No. 2, February 15, 1919.

FIRST "BAD HABIT"—SUCKING ITS THUMB

Fig. 11.

DAVID FAIRCHILD

HOW is it to be explained that these babies, when still too young to imitate what is going on around them and without being taught, acquire what many doctors and dentists declare to be a very bad habit? Baby mud-wasps which hatch after their mothers and fathers are dead know how to build their complicated mud nests without being taught. Are we sure that this human habit of thumb-sucking, which has appeared successively generation after generation in very young babies, is not of a similar instinctive or hereditary character?

If it is hereditary, is it beyond question injurious, and does it certainly produce protruding teeth and flattened thumbs, and does it arch the roof of the mouth, or are these changes merely coincidental with the habit? Are we confident that by employing forcible means to cure the habit we do not run the risk of affecting the child's emotional or moral nature—of making it secretive or even untruthful? Should not the habit be cured by the ridicule of a child's peers on the playground—by its own self-control?

If this habit is hereditary, are not others of the same nature, and should we not, in our discipline of children, differentiate between the habits of imitation and those which are inherited?

According to Dr. Alexander Graham Bell:

> The mere act of suction could produce no such effect as the protrusion of the teeth. On the contrary, with a partial vacuum in the mouth the atmospheric pressure from outside would tend to push the teeth *in*, not out. Theory would indicate that the act of suction should actually be beneficial to the plastic growing mouth by bringing atmospheric pressure into play from outside, tending to consolidate the mouth and oppose any tendency to the spreading of the parts.
>
> Any spreading action could only be due to pressure applied from within. A child for example might press his tongue against the roof of the mouth or against the back of the front teeth and thus produce a pressure which if constantly applied would gradually result in a spreading action, but there is no evidence that a child ever does this.

Again it is conceivable that a child in the intervals between suction might press his thumb against the back of the front teeth but here again there is no evidence of active muscular effort. On the contrary the intervals between the acts of suction are characterized by relaxation of the whole muscular system, the child tending to fall asleep and the thumb tending to fall out of the mouth.

Sucking the thumb is such an instinctive action with very young children that it is probable that most babies, if not all, indulge in the habit during the first year of life. Some people have protruding teeth in adult life and if you examine their past history it is probable that most of them sucked their thumbs in infancy. Hence we are apt to jump to the conclusion that the sucking of the thumb was the cause of the protrusion of the teeth, in spite of the fact that will be at once perceived by those who know anything about atmospheric pressure that the act of suction could not possibly produce any such effect.

The idea that suction could produce a spreading action in the mouth is as ridiculous as the very common notion that sucking the thumb causes a child to become cross-eyed!

The importance of being able to make or break a habit at will was so drilled into me by one of my great friends that it has come to seem to me one of the greatest things in life. When the first baby came I determined that he should have a fair start. He should not be allowed to form any undesirable habit. Almost immediately, or certainly when he was only a week or two old, he got his thumb into his mouth. Everybody admitted that this was the beginning of a bad habit. Some predicted that he would make his front teeth protrude and spoil the shape of his mouth, others knew he would ruin the looks of his thumb, and others said that thumb-sucking was connected with internal disorders of a serious character.

With the impression firmly implanted in our minds that the forming of a bad habit was the worst thing that could happen to a baby, we began to try to cure this one of sucking his thumb. We tried cassia solution, but he enjoyed the bitter taste. We put tiny mittens on his hands, and he either sucked them until they were wet through or pulled them off. We tied his arms, but he always struggled loose. We bought those round aluminum hand balls which are supposed to be a real cure, but he bumped his head with them and got into almost hysterical fits of temper.

The weeks stretched into months and we did not give up the fight. As the months became years we tried bribery and corruption. We threatened and we punished. When the second child came she followed in her brother's footsteps, with variations, and when the third baby arrived she embroidered the thumb-sucking habit by twisting her night dress or sheet around her third finger. By this time we began to realize that we were confronted by something which was not so simple as one of the habits of later life, and we finally gave up. There seemed to be something almost instinctive in the way this habit resisted our treatment, and we began to suspect a hereditary character in it.

Among some old photographs of the children's mother (perhaps luckily for the father none of his own baby photographs were on hand) we came across one in which she was shown sucking her thumb. By a curious coincidence we had had one taken of our eldest child at about the same age and in the same attitude without knowing of the existence of the mother's photograph. And while we were discussing this, it came up in conversation that the mother's mother had also sucked her thumb, and later the daguerreotype evidence of this was found.[1]

The evidence seems strong to his parents that it was natural for the boy to suck his thumb and did not come from sheer wilfulness on his part.

If this is indeed an inherited instinct and not a personally acquired habit, the breaking of it may be a long and difficult task, and we should be very sure that it is indeed injurious before we waste our time and worry our poor little babies trying to do so.

What a weight would be lifted from the shoulders of thousands of young parents could they with clear consciences let their babies suck their thumbs in peace!

[1] The photographs here published, with the permission of all three babies, it is understood, are of Mabel G. Hubbard (Mrs. Alexander Graham Bell), Marian Hubbard Bell (Mrs. David Fairchild), and Graham Bell Fairchild. Each of these three babies has its right thumb in its mouth.

AN EARLY FAMILY HISTORY OF COLOR BLINDNESS

Illustrating the Rule That Sons Can Inherit Normal Color Vision Only from Their Mothers, While Daughters May Inherit It from Either Parent or Both

LEON J. COLE
Professor of Genetics, University of Wisconsin

IN THE Philosophical Transactions of the Royal Society of London for 1779 is recorded a rather complete family history of color blindness, to which, so far as I am aware, attention has not been called in recent studies on the subject. It consists of a letter from one J. Scott, describing his own condition and that of his near relatives, which was communicated to the Society by the Rev. Michael Lort.[2] It is of interest because of its early date and because the facts as given fall so perfectly into line with the interpretation of color blindness as a sex-linked character in man.

In describing the imperfection in his own sight Mr. Scott states that he does "not know any green in the world." Pink and pale blue appeared alike; full red and full green the same. He could tell yellows and all degrees of blue except very pale. He was sometimes baffled by full purple and deep blue. In this connection he cites an incident which is so quaint and full of human interest that it may be quoted verbatim. He relates:

"I married my daughter to a genteel, worthy man a few years ago; the day before the marriage he came to my house, dressed in a new suit of fine cloth clothes. I was much disappointed that he should come (as I supposed) in black: said, He should go back to change his colour. But my daughter said, No, no; the colour is very genteel; that it was my eyes that deceived me. He was a gentleman of the law, in a fine rich claret-coloured dress, which is as much a black to my eyes as any black that ever was dyed."

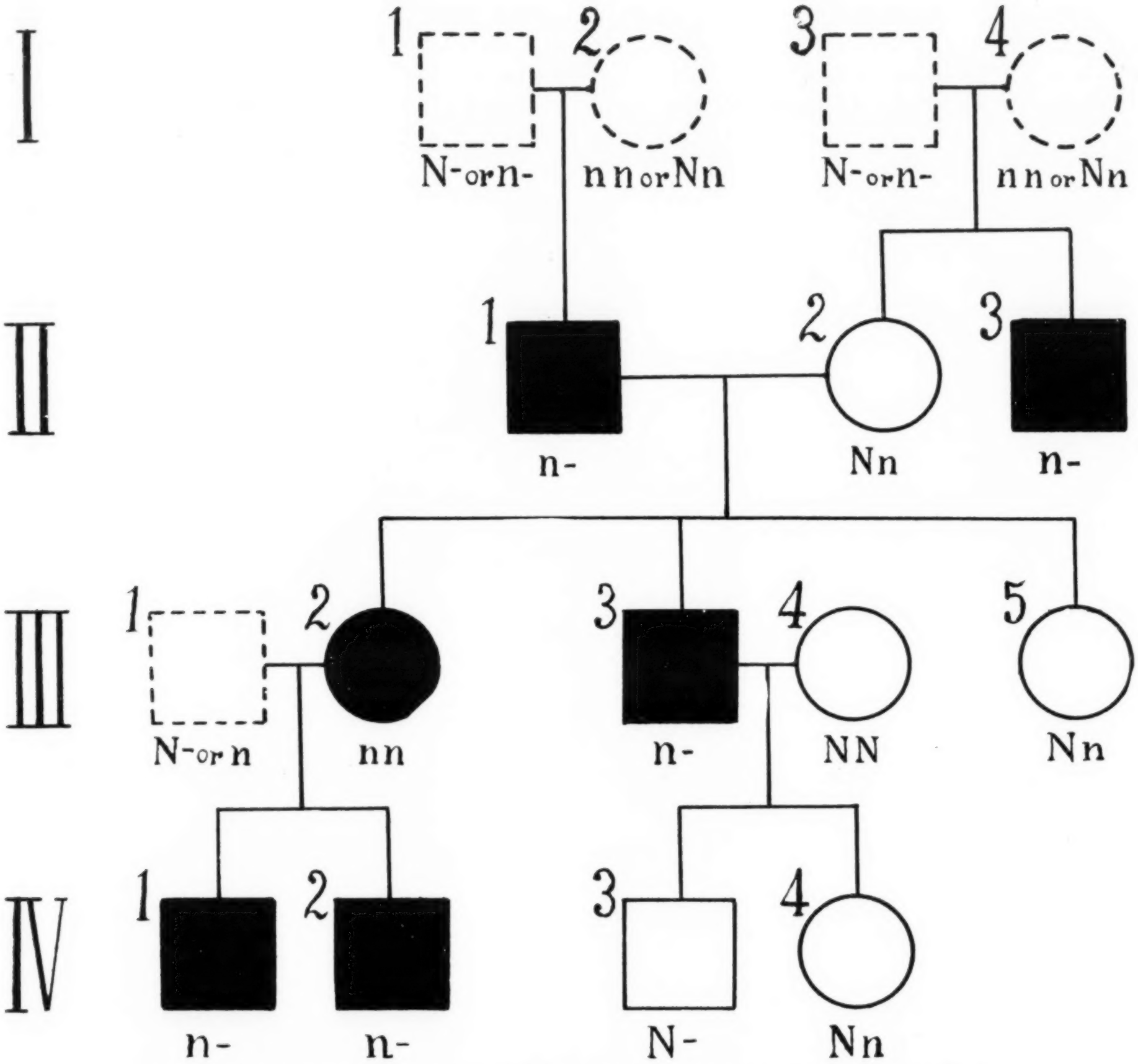
The accompanying diagram is constructed from the data supplied by Mr. Scott on the occurrence of color blindness in his family. In the following quotation the numbers inserted in brackets show the position of the individual referred to in the diagram. To use then his own words:

"It is a family failing: my father [II 1] has exactly the same impediment: my mother [II 2] and one of my sisters [III 5] were perfect in all colours: my other sister [III 2] and myself [III 3] alike imperfect: my last mentioned sister has two sons [IV 1, 2] both imperfect: I have a son [IV 3] and daughter [IV 4], who both know all colours without exception; and so did their mother [III 4]: my mother's own brother [II 3] had the like impediment with me, though my mother, as mentioned above, knew all colours very well."

Using the symbol *N* for normal vision and *n* for its allelomorph, and also assuming that *N* is sex-linked, we may have the following genotypes: *NN*, homozygous normal female; *Nn*, heterozygous normal female; *nn*, color blind female; *N*—, normal male; *n*—, color blind male. It is then possible, on the basis of the data given, to designate the genotypes of all the individuals men-

[1]Papers from the Department of Genetics, Agricultural Experiment Station, University of Wisconsin, No. 17. Published with the approval of the Director of the Station.

[2]Lort, Michael. An account of a remarkable imperfection of sight. In a letter from J. Scott to the Rev. Mr. Whiffon, of Trinity College, Cambridge. Communicated by the Rev Michael Lort, B.D., F.R.S. Philos. Trans. Roy. Soc. Lond., Vol. 68, Part 2, pp. 611-614, 1779.



THE OCCURRENCE OF COLOR BLINDNESS IN ONE FAMILY

The above diagram illustrates the history of color-blindness as it occurred in three successive generations. The father was color blind, and the impediment was inherited by his son and one daughter. The mother and one daughter had perfect eye-sight. The daughter who was color blind had two sons, both with imperfect eyesight. The son also had two children, a boy and a girl, who both, like their mother, had normal color vision. (Fig. 12.)

tioned. For the males this is of course simple, since it may be known from the phenotype, all normal males (IV 3) being *N*— and the color blind ones *n*—(II 1, 3; III 3; IV 1, 2). In the case of the normal females, on the other hand, the genotype must be ascertained from the known kin. The subject's mother (II 2), for example, was normal, but since she had both a color blind son and daughter she must have been *Nn*, that is, a "carrier" of color blindness; the son must have inherited the color blindness from his mother, since it is transmitted only in that way, and a daughter can be color blind only when she inherits it from both parents. The other daughter (III 5) was normal, but must have been heterozygous (*Nn*) because of her father being color blind. The same rule applies to the subject's own daughter (IV 4). The only other woman described is the subject's wife (III 4), who had normal vision, and who probably was homozygous (*NN*), for had she been heterozygous the chances would have been even for either the son or the daughter to be

color blind, and considerably less than even that they would both be normal.

As to the conditions of the subject's grandparents (I 1 and 2, 3 and 4), which are not stated, it is possible only to conjecture, though it is apparent that both the grandmothers must have been either color blind or heterozygous for the defect. The grandfathers might have been color blind or not as far as the data go. Nothing further can be said about the paternal grandparents, but the subject's mother having had normal vision it is possible to say that if his maternal grandmother (I 4) was color blind her husband must have had normal vision, whereas if she had normal vision he must have been color blind. That is, to give the results obtained (heterozygous normal daughter and color blind son) the mating must have been N— $\times$ *nn* or *n*— $\times$ *Nn*.

It would have been interesting if he had noted the condition of his brother-in-law (III 1), that is, the husband of his color-blind sister (III 2). It was a foregone conclusion that all her sons would have defective vision since she herself was color blind, but any daughters would have served as an index to the father's condition, since their vision would have corresponded to his. As it is, he may or may not have been color blind.

THE GESTATION OF THE CARABAO

B. M. Gonzalez,

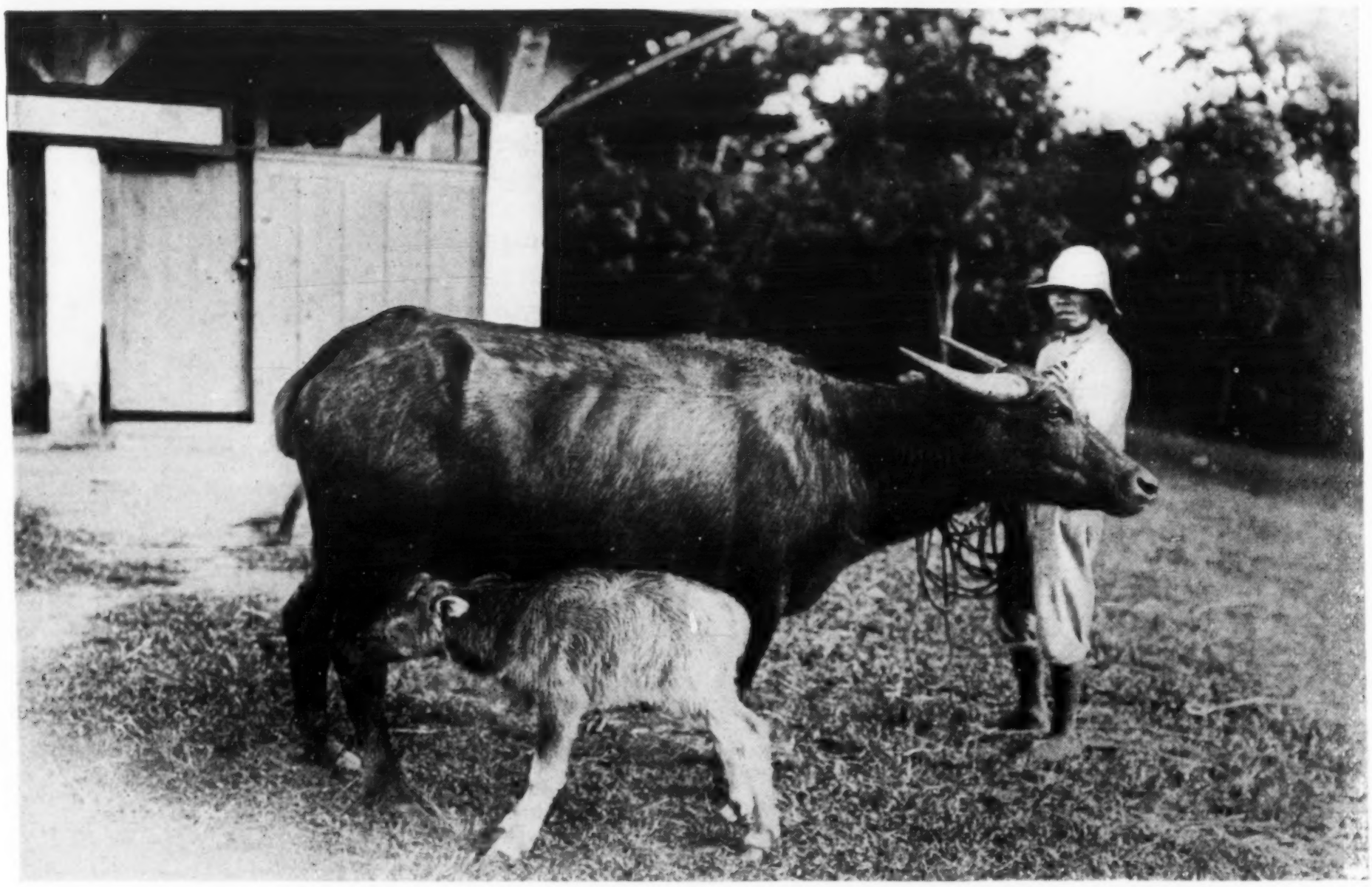
Associate Professor of Animal Husbandry, College of Agriculture, University of the Philippines.

LITTLE is known about the breeding habits of the carabao (*Bos bubalus*), since these animals are on the range most of the time. They rarely go under shelter except during the busy season of harvesting, when they are tied close to the house of the farmer, under a light bamboo and cogon or nipa shed, and are fed on sugar-cane leaves or rice straw. While the crops are growing, the carabaos are used in cultivating the crops, and are grazed on vacant lots, on paths overgrown with grass, or on the roadside. At these times breeding is not likely to occur, as these animals are herded more or less individually by the children of the farmers.

After harvest, all the carabaos in the neighborhood have the run of all the fields in common, since fenced areas are rather rare in the Philippines, and it may be presumed that it is at this period when breeding occurs. Carabaos mature later than cattle, bulls hardly breeding regularly before three years of age, and cows usually bringing their first young at five, sometimes at four, but rarely before that. Under such conditions it is impossible to determine the exact gestation of the carabao, and an inquiry among the farmers elicits a variety of answers ranging from nine to thirteen months or more.

With the acquisition of a small herd of these animals by the College of Agriculture, the cows were kept separate from the bull from the start, and observations were made on their estrum. The period of estrum of these animals is very obscure, and the only way we succeeded in breeding them was by attempted matings of them at frequent intervals.

The figures obtained so far, while few, are significant, inasmuch as they are fairly uniform. Due to the reasons mentioned above, observations on the period of gestation of this animal will probably never become numerous. At the College we estimate that we lost about half a year on an average for each cow observed by not knowing the exact time when each cow should be bred, due to the obscurity of estrum.



CARABAO COW WITH THREE DAYS OLD CALF. (Fig. 13.)

The Philippine carabao is essentially the same as the buffalo or water buffalo of India, China, Java, and other oriental countries, except for slight variety differences as to size and shape of horn, color and texture of skin, and slight differences in conformation. The writer examined the literature from the above countries but has not come across any observations pertaining to this question.

The animals observed so far are as follows:

The gestation of the carabao, therefore, is approximately 320 days, or forty days more than the cow (280),

Animal.	Date bred.	Date calved.	Sex of calf.	Period of gestation
Cow No. 1.....	June 24, 1918.....	May 14, 1919.....	Female.....	324 days.
Cow No. 2.....	Jan. 5, 1918.....	Nov. 19, 1918.....	Female.....	318 days.
Cow No. 4.....	Jan. 6, 8, 10, 1918...	Nov. 30, 1918.....	Male.....	324–328 days.

and twenty days less than the mare (340).

Observations were also made on the period of estrum immediately following parturition, and this gave us:

This period was determined by trying, every two days after calving, to mate the animals. These latter figures are simply offered as observations and may not hold true in every instance.

Animal	Date of calving	Date of first estrum	Lapse of time after calving
Cow No. 2.....	November 19, 1918....	Jan. 12, 1919.....	54 days
Cow No. 4.....	November 30, 1918....	Jan. 23, 1919.....	54 days

A REMARKABLE BUD SPORT OF PANDANUS[1]

JOHN H. SCHAFFNER

A SMALL specimen of Pandanus, planted in a flower pot, had been carrying on a precarious existance for many years in the botanical greenhouse of the Ohio State University, when in the fall of 1916 it was transplanted into a bed of rich earth in the aquatic room. This screw-palm apparently belongs to the species *Pandanus utilis Bory*. The genera of Pandanaceae—Freycinetia, Sararanga, and Pandanus—are all described as having spirally arranged leaves and apparently all have three spirals. It is not certain, however, from the descriptions at hand, whether or not all the species have the three spirals.

The plant under consideration had the normal three spirals of leaves characteristic of the species and continued this character for awhile after being transplanted; but the leaves immediately began to grow larger and longer, probably due entirely to the improved soil and water conditions. Soon, however, the terminal growing bud underwent a remarkable transformation and sported in such an extreme manner that the plant has assumed an entirely new aspect. The mutation resulted in a two-ranked arrangement of the leaves in which the spiral twist is entirely absent (see Fig. 14). After growing for some time, the plant took on the aspect of a great fan, putting one in mind of a traveler's tree, *Ravenala madagascariensis*. The last three leaves before the change were each about 2 feet 1 inch long and quite rigid. The first two leaves of the two-ranked portion were also 2 feet 1 inch long and rigid, but in the subsequent stages the leaves continued to become longer and very flexible and recurved. The eighth and ninth leaves were each 4 feet 3 inches long, while the latest leaves produced measure 5 feet 3 inches in length.

The families, Sparganiaceae and Typhaceae, are closely related to the Pandanaceae, and in passing up the series to Typha one finds not only a much more specialized type of flower but also the more extreme conditions represented in the herbaceous stem, geophilous habit, two-ranked leaves, and distinct internodes. The leaves of Typha are still somewhat spiral in arrangement, but have the prominent sheath characteristic of many of the highly evolved monocotyls.

The Pandanales are probably more closely related to the lower palms than to any other group of plants, and the palms approach such lower lily types as Yucca and Cordyline. Among the lilies and their relatives the change from three to two spirals or to the two-ranked condition is frequent, and takes place even at the lower levels of the evolutionary series.

The palms are well known as mostly trees with unbranched trunks and three spirals of specialized leaves. Assuming such an origin for the Pandanales, the phyletic arrangement of the three related genera is as follows: Pandanus, Sparganium, Typha. We have, then, the following progressive series:

1. From tree to geophilous, rhizome herb.
2. From three spiral leaves to two-ranked condition with the spiral more or less obliterated.
3. From no internodes to well-developed internodes.
4. From fleshy drupes to minute, dry, nut-like fruits.

[1] Papers from the Department of Botany, the Ohio State University, No. 113.

LEAVES OF PANDANUS

This unusual specimen of Pandanus originally had the three spiral leaves characteristic of its species, but after being transplanted from a flower pot into a bed of rich earth, transformed its leaves into two ranks with the spiral twist entirely absent. The leaves which were at first 2 feet 1 inch long and quite rigid grew to a length of 5 feet 3 inches and became very flexible. (Fig. 14.)

The present bud sport of Pandanus utilis has therefore made two fundamental progressive changes at one mutation. It has lost one spiral of leaves and also has changed the positions of the two remaining ranks in such a manner that the spiral or twist is entirely obliterated. The cause for those peculiarities must be in some changes in the nature of the hereditary apparatus, presumably the chromosomes, but whatever the cause, it is of the same nature as that which must have occurred a great number of times either in the vegetative tissues or in the reproductive cells of numerous independent series of Monocotyls. In some groups one of these characters has been acquired independently. In those cases where a species has advanced to the two-ranked condition without a spiral twist there may have been but one mutation, as the bud-sport in hand, or there may have been two or more successive mutations to bring about the final form.

Beyond the Veil

MODERN PSYCHICAL PHENOMENA, by Hereward Carrington, Ph.D. Pp. 331, illustrated, price $2.50. New York: Dodd, Mead & Co., 1919.

Psychical research is becoming ambitious. Not content with lifting the veil that conceals the future, it will now raise the veil from the past. Thus Dr. Carrington not only elucidates the nature of sex and bisexual reproduction in the spirit world of after-death, but illuminates the origin of natural selection.

Life appeared on this planet when the conditions "were favorable to the formation and continuance of life." So far, so good. "So long as these conditions lasted, life prospered; everything favored its growth. This first living matter grew and finally split up, or in some manner gave birth to other living matter, and the procession of life had begun." Simple, like all other great discoveries.

"But mark this—death had not yet appeared upon the earth; sickness and decay had not yet made their presence felt—for if they had, at the very origin of creation, life would have become extinct as soon as it came into being." The argument is at least plausible.

"So long as life went on in an uninterrupted and peaceful manner, just so long was 'everything in the garden lovely.' Life increased and multiplied, and nothing had come to destroy it. But one fine day the living substance reacted wrongly—contrary to the laws governing its environment, and then the external forces of nature burst forth, swept down and destroyed a portion of this living substance, and injured another portion, causing it to become 'diseased' and decay. The uninjured portion continued to live and propagate; the injured portions died." *"Disobedience to cosmic law* thus constituted the first evil—*protoplasmic disobedience*—the penalties for which were disease and death."

Aside from such evolutionary revelations, Dr. Harrington's book contains a readable account of the status of psychical research as viewed by one whom the professional researchers regard as an authority. He considers "hypnotism, dreams, telepathy, crystal gazing, automatic writing, the vast powers of the subconscious mind, dissociation of personality, multiple personality, and many other phenomena today recognized and accepted by 'orthodox' science," and believes that "many other phenomena (such as apparitions, 'ghosts,' haunted houses, telekinesis, materialization, thought photography—even spirit communication itself) are today gradually but surely winning acceptance." His ideas of the valuation of evidence may be measured by his discovery of "a mathematical proof of the existence of a spirit world" in the fact that mathematicians find "imaginary quantities" convenient in their notation.

THE RELATION OF HEREDITARY EYE DEFECTS TO GENETICS AND EUGENICS[1]

LUCIEN HOWE, M.D., *Buffalo*

OF LATE years, while trying to learn something about ocular muscles, I have been confronted often by questions concerning heredity. I have found, as others doubtless have, three, four or even more persons in the same family with a similar form of heterophoria,[2] heterotropia,[3] predisposition to ocular fatigue, or similar abnormal muscular conditions. It seemed impossible to study these anomalies satisfactorily without first halting to learn something about that mystery which we call heredity.

It is not possible here to give any systematic account of the studies already made of hereditary eye defects, except to mention the classic work by Groenouw,[4] the interesting histories collected by Nettleship, the more recent bibliography by Loeb, with the exhaustive articles by him, or those by Libby and others. The point is that we ophthalmologists have been content thus far with reporting family histories without attempting to relate those histories to other facts, now well established by geneticists.

The best way to learn these principles, and one vastly more interesting, is to supplement the reading with at least a few experiments.

The breeding of eye defects is easier than most persons imagine. Chickens and pigeons are the best subjects for such experiments.

By advertising in the *Reliable Poultry Journal* and other trade papers, it has been possible to obtain for the parent stock more than a dozen specimens of eye defects. These included corneal irregularities, and variations in the color of the iris and in the position of the eyes and of the pupil. The different pens of chickens at a small place, known as Mendel Farm, on the lake shore near Buffalo, have proved to me a source of much interest and enlightenment.

The breeding of dogs has not been found satisfactory. Eye defects are rare, the generations slow and the litters small. For similar reasons, cats are undesirable. It is probable, however, that interesting results could be obtained by breeding white cats which have blue eyes—such cats being often deaf. This fact had been already observed by Darwin and has been the subject of breeding experiments by Dr. Graham Bell.[5]

The eyes of the small fruit fly, *Drosophila ampelophila*, have proved most interesting. I am indebted to Professor Morgan of Columbia for parent stock, the blind variety of which I bred through more than twelve generations.

PRACTICAL VALUE OF THE STUDY

The average reader of this paper may say that such a glance at the principles of heredity may be curious and possibly interesting, but of what practical use is it?

Of course we never find a typical Mendelian ratio in the human species, because brothers and sisters never

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[1] Read before the Section on Ophthalmology at the Sixty-Ninth Annual Session of the American Medical Association, Chicago, June, 1918. Reprinted in condensed form from the *Journal of the American Medical Association*, Vol. 70, pp. 1994–1997.

[2] A tendency of the visual axes to fail to meet in the fixation point, due to weakness of one or more of the ocular muscles or their faulty innervation.

[3] Displacements in position.

[4] Groenouw, in Handbuch der gesamten Augenheilkunde, Graefe-Saemisch, Ed. 2, Part 1, Vol. 11, p. 415.

[5] Bell, Alexander Graham: Tr. Otol. Soc., 1885, Vol. 3, p. 478.

marry.[6] But an approach to these ratios we do find constantly, and that law furnishes an important guide, not only in recognizing heredity itself in a given family history, as distinguished from an infection, but in also indicating when the defect is dominant or recessive. In other words, this relation between genetics and ophthalmology throws light on the differential diagnosis (the next step in our study), and is very decidedly practical in connection with eugenics.

With this reassurance as to the reason for continuing, let us pass to the examination of family histories to determine whether a given defect is in reality hereditary, or whether it is due to infection from syphilis or other causes.

It would necessitate a rather long digression to show just how this differential diagnosis is made. Suffice it to say that our criterion is the technical definition of heredity given at the outset. The fact is that a very considerable proportion of defects which we all supposed formerly were hereditary are probably not hereditary, but the effect of some infection more or less obscure. An excellent example of this is a family history of aniridia reported by one of the oldest and best known members[7] of this section. The author and all other ophthalmologists accepted this as an undoubted example of heredity, but competent geneticists now demonstrate that to be highly improbable.

The term "hereditary" we should also define more exactly as including two groups--one in which the recurrence of the defect is only "possible," especially when that defect is "recessive," the other in which its recurrence is "probable," especially when that defect is "dominant." Evidently, therefore, we have as many conditions to deal with as there are combinations, in pairs, of these four factors.

Still another factor enters into the problem—the personal equation of the parties to a marriage contract. For when two persons contemplate matrimony, it can usually be taken for granted that their judgment has, for the time, taken wings. Professor McCready, in his lectures at the College of Physicians and Surgeons, used to say his case records showed that for various reasons he had advised one or both parties against marriage some sixty-eight times; and his records also showed that sixty-eight times the couples went almost straight to the altar.

But temporary obsessions by those who ask an opinion is no excuse for inability to advise intelligently, or, if necessary, to restrain persons from a fatal mistake. Therefore the other four factors in our problem demand all the more careful study. While the difference between a simple deformity and blindness is evident, it is not easy to say whether the recurrence of a given defect is "possible" or "probable." Our decision, then, must be based on three groups of data: first, whether the defect is inherited as a dominant or a recessive; second, whether it is a sex-linked characteristic, and, third, whether both families show the defect.

A few illustrations will show how these four cardinal conditions in our problem may serve as the basis for an opinion by an ophthalmologist when he is called on to give advice in the case of a proposed marriage. Thus:

(*a*) When we have to do only with a deformity, and its reappearance is doubtful, no special objection need be raised.

(*b*) When we have to do only with a deformity, and its reappearance is probable, the parties to the marriage should both be warned of the probability of the reappearance of that defect in their offspring.

(*c*) When we have to do with blindness, and its reappearance is doubtful, a stricter attitude toward the union can be assumed.

(*d*) When we have to do with blindness, and its reappearance is probable, the question assumes its most serious aspect. If the family history shows

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[6] Risley, S. D.: Hereditary Aniridia, *The Journal Amer. Med. Assn.*, April 17, 1915, p. 1310.

[7] Matings between brothers and sisters are not essential to the formation of Mendelian ratios. Ed.

that this defect is in reality dominant, then half the children on the average will develop the defect; or even if the defect is recessive, but occurs in both families, the danger is practically the same.

Another phase of the same problem is presented when a couple is already married, and with little or no history on either side of eye defect; yet in spite of that, for reasons which we do not know, one child after another, blind or partly blind, is born to these parents. This congenital blindness is in one way quite different genetically from hereditary blindness, but from the standpoint of eugenics, families with hereditary and with congenital blindness may be considered together as one group and classed as hereditary. It is desirable to appreciate how important this group is, but space permits here only the categorical statement of a few facts, which, properly presented, would fill a small volume. These facts are:

(*a*) The number of this group in the United States is considerable. It has been estimated at about 8,000 and upward. According to the census of 1910, the total number of blind was 52,272. Counting unavoidable and admitted errors in that census, the results of subsequent surveys, and increase of population, the National Committee for the Prevention of Blindness estimated the number of blind in 1917 at from 100,000 to 110,000. According to the data given by Best in his exhaustive and recent study of the blind in this country, the proportion of hereditary and congenital blind is considerably larger than here stated. But in giving estimates, the minimum limit is evidently the safest.

(*b*) The cost also is considerable. According to statistics carefully collected in Massachusetts and elsewhere, the average cost per year of each blind person in and out of institutions is at least $475. This gives us a total annual cost of some $3,800,000.

(*c*) It is unjust to the blind to allow them to be brought into existence simply to lead miserable lives.

(*d*) It is unjust to taxpayers to be compelled to support them.

(*e*) The longer we delay action to prevent this blindness, the more difficult the problem becomes.

(*f*) A large part, if not all, of this misery and expense could be gradually eradicated by sequestration or by sterilization, if the transmitter of the defect preferred the latter. An idea of this plan is given by the somewhat similar one for dealing with feeblemindedness. In 1913, laws for sterilization, under certain circumstances, had been passed in twelve states and proposed in nine others. Bulletin 10 of the Eugenics Record Office, Cold Spring Harbor, L. I., N. Y., gives a map showing the status at that time of such legislation. Where such eugenic laws have been enacted or favorably considered, probably existing laws could be properly amended or new laws passed to prevent this form of blindness. The same bulletin gives the form of a model law for this purpose. It was prepared by committees composed of the most eminent physicians, surgeons, sociologists, students of genetics and similar experts in this country. That law could be made applicable to the prevention of hereditary blindness, but with an important difference, which makes the prevention of this form of blindness comparatively easy and inexpensive. No legal action is essential until after a marriage has been found to produce blind children, and even then sequestration of the transmitter of the defect may be substituted for sterilization. Moreover, if this sequestration is of a kindly nature, with congenial occupations suited to the intelligence and social tastes of the subject, the sense of restraint is slight, since it is reduced to the minimum. This is not theory but experience, as shown especially by the colonies for the feebleminded established by Dr. Charles Bernstein of the State Custodial Asylum at Rome, N. Y.

WHAT ARE WE GOING TO DO ABOUT IT?

In other words, we have all this misery and expense, both of which are largely unnecessary; we have also an effective method of relief prepared by committees of our foremost experts and already well supported, in principle, by

public opinion. What are we going to do about it? That is the question at last forced on ophthalmologists—especially on the Section on Ophthalmology of the American Medical Association. Shall we complacently continue to do nothing, thus retarding social advancement and even ophthalmology itself? The answer to this question involves many details, both of principle and of method, far too complicated for discussion now. But in executive session a motion will be made for the appointment of a committee to report on this subject another year.[9] If at that time or later, some plan can be formulated for the prevention of this form of blindness, it will perhaps seem worth while for us to have directed our attention now, even in this hasty manner, to the relation of hereditary eye defects to genetics and eugenics.

A STRAIN PRODUCING MULTIPLE BIRTHS

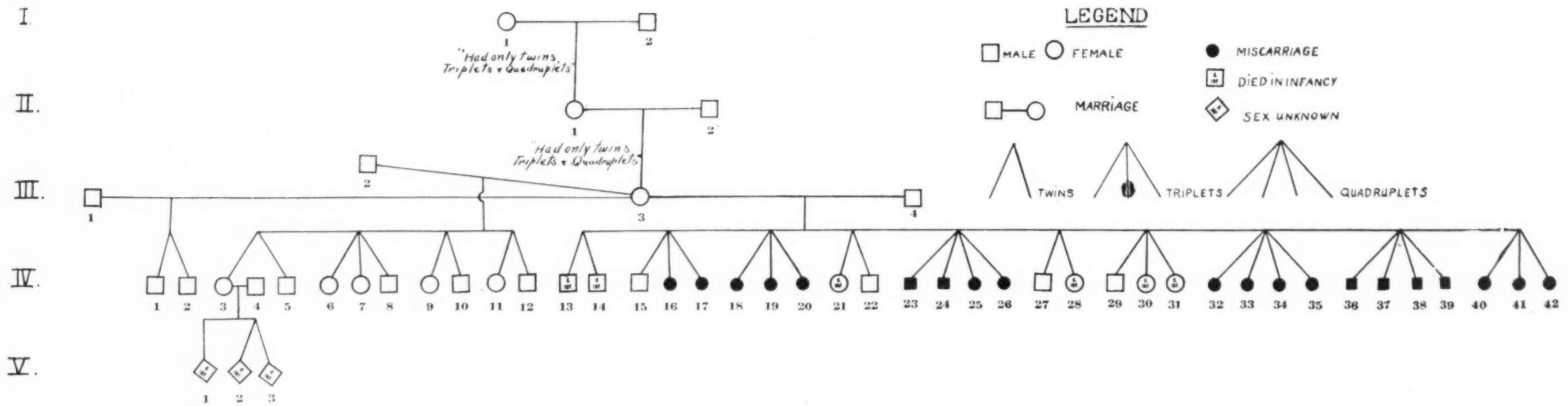
C. B. Davenport, *Cold Spring Harbor, L. I.*

AMONG the findings of the Eugenics Record Office has been that of a family in Cleveland (Mrs. W. G. C.) which has a remarkable record in the production of two or more children at a birth. The history has a special interest owing to the fact that the present Mrs. C. has married three times. There is no information at present available concerning the tendency toward multiple births in the families of the husbands, but, in view of the fact that multiple births are relatively rare, it is exceedingly improbable that all three husbands belong to families having such tendencies. The propositus (Mrs. C.) has been interviewed at different times by three representatives of the Eugenics Record Office. To each the history given has differed in some details. The last of the field workers who visited the woman secured the facts concerning her first husband and her children by him which had remained unknown to the other field workers, and were indeed divulged only after five visits had been made by the last field worker. Further studies are being made upon the family, but it seems probable that the facts are now so nearly ascertained as to warrant a description of it.

The details of the family are set forth in the accompanying figure (Fig. 15), which is a pedigree chart for five generations. The propositus is No. 3 of the third generation, III 3. Her mother and her mother's mother are said by the propositus to have had only twins, triplets, and quadruplets. It must be said in all candor that the propositus, although willing to coöperate, has certain mental limitations which make it necessary to check all her statements from independent sources, and this has not been possible in the case of the statement concerning her mother and mother's mother, as the propositus was born in Paris and her parents and grandparents never came to America.

The propositus married first husband No. 1, and by him had twins (IV 1, 2) but her husband, Mr. M., died fourteen months after their marriage. Two years after her first marriage, the propositus married T. R., who was a French Canadian, and who died in 1896. By him the propositus had first children, twins, Violet and Clay. This Violet married a Mr. N., and had a single child, and two years later twins, who with the mother died shortly after birth. The next children of the propositus were triplets, named Esther, Flossie and Theodore. These all died

[9] That committee was appointed and we have begun the work.



PEDIGREE CHART FOR FIVE GENERATIONS

The diagram above illustrates a remarkable record of "a woman who, in three successive marriages, has never had a single child at a birth." A history of this case shows that there have been multiple births in each of four successive generations. The propositus who is indicated by No. 3 in the third generation (III-3) was married three times. (Fig. 15.)

young of some intestinal disorder. Two years after (in 1895) there were again born to the propositus twins—Anna, who still survives, and Charles, who died young. The next year another pair of twins were born—Irene, who still survives, and Clarence, who died young. The propositus married, third, W. G. C., in 1902, and he is still living. He is of English and Scotch descent. By him the propositus had first (probably 1903) a pair of twins by the names of Howard (who died at eight months of hydrocephalus and Noah (who died soon after his twin brother). In December, 1904, triplets were born, of whom the eldest, Will, is still living, but of whom the other two were born dead. About a year later the propositus had a miscarriage of triplets (IV 18, 19, 20). In 1907 there were born twins (IV 21, 22); a boy, Hugh, is still living, but the girl died one hour after birth. Then followed the next year a miscarriage of two boys and two girls. This seems to have been brought on by the poor health of the mother. In 1909 there were born twins (IV 27, 28); a boy, Edward, is still living and a girl died at the age of ten days in the hospital. In 1910 triplets were born (IV 29, 30, 31); a boy, Roland, still lives at home, one of the girls died at the age of one week, and the other was a "blue baby" (that is, one in which the foramen ovale failed to close). In 1912 the propositus gave birth to four girls, born at full term in March, and in December of the same year, she had a miscarriage at about five months, associated with a severe burn that she experienced. In April, 1913, the propositus had another miscarriage, at about three months, of females, as indicated on the chart IV 40, 41, 42. Thus the propositus has averaged nearly three children at a birth and has had thirty pregnancies inside of twelve years in the last mating, in addition to the eleven by the two earlier matings.

The foregoing remarkable case is by no means unique, but accords very well with the similar cases that are reported by Gould and Pyle, "Curiosities of Medicine." The present case, however, is of especial interest because of the testimony that the tendency to multiple births has appeared in each of four successive generations. That the tendency to multiple births is hereditary is clearly brought out in the case of sheep in strains which, like that of Dr. Alexander Graham Bell, have been developed in which twins are almost exclusively produced. Even among commercial breeders the existence of such twin-producing strains is well known. The Dorset horned sheep are reputed to show a greater tendency toward twinning than other races of sheep. It is hoped to make further studies upon the case described in this communication.

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